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EXPERIMENTS WITH SELENIUM, AND WIRELESS TELEPHONY BY MEANS OF LIGHT RAYS.

PAPER BY MR. A. ROSENBERG, *Member.*

Read before the Optical Society, on November 20th, 1902.

WHEN Mr. Pharaoh, our energetic Honorary Secretary, did me the honour to invite me on behalf of the Council of this Society to read a paper this evening, I was at first very much at a loss for a subject; but I eventually decided on a short talk about wireless telephony by means of light rays as the topic on which I will venture to ask your attention.

Wireless telephony by means of light rays requires a transmitter and receiver, as in wireless telegraphy by means of Hertzian waves (which are really light waves of a slower periodicity), but instead of using a transmitter for generating invisible light rays and a "coherer" for detecting them, in this case we use an arc lamp which sends out *visible* rays and a so-called selenium cell for detecting these rays.

The invention both of the musical or speaking arc lamp and of this system of wireless telephony which I hope to demonstrate to you is due to Prof. H. Th. Simon, of the University of Erlangen, and it was due to the discovery he made in 1898, that every time in a neighbouring room an induction coil was used an arc lamp fed by a continuous current repeated in the arc the sound of the break on the induction coil; and on looking for the cause of this phenomenon he found the explanation in the fact that the wires feeding the coil ran for a short distance parallel with the wires feeding the arc lamp; and as he found that the arc was sensitive to even such very weak induced currents he tried to utilise in the arc the weak currents from a telephone circuit, and by that means (he reasoned) he could reproduce in the flame of the arc the sound emitted before the microphone. His experiment succeeded, and proved to be the origin of the speaking or musical arc lamp.

When this discovery was announced to the world, a great number of investigators set about to improve it, as at that time the sound emitted from the arc was so faint that, in order to hear it, ear tubes not unlike those used for the phonograph had to be employed, with a small glass funnel at the outer end.

Improvements soon followed, notably the systems evolved by Dudell, Ruhmer, Brauwn, and others; and the improvements aimed at were mainly in the direction of making the delivery of sound louder and clearer.

As time will not permit me to enter into the theory and construction of the various systems, but only to explain the fundamental principle which applies to nearly all speaking arc flames, I will just say that they are fed by a continuous current; the undulating microphonic currents are super-imposed on the current feeding the arc, which is so sensitive that the small variations in the strength of the induced currents cause sufficient temperature variations in the arc, as also variations in the valume of the conducting gases and amount of carbon in the arc. So as to make the sound emitted by the arc a maximum it is desirable that the amplitudes of the induced current feeding the arc should be as big as possible. I will now proceed to give you some experiments with a lamp that I designed some three years ago.

In order to hear the sound emitted from the arc without at the same time hearing the sound impingent on the microphone, I have the microphone in another room, connected by wires to the transformer feeding the arc. I have also a phonograph in that room; and my assistant will set the phonograph in motion, when the sound of the phonograph given out before the microphone will be reproduced loud enough for the whole audience to hear. (Here follow some three or four selections.)

Without a doubt the speaking arc is one of the most interesting and wonderful inventions of modern times, and at this stage it will suggest itself to those in the audience acquainted with the interesting experiments Bell made with the photophone, to utilise this lamp for wireless telephony; but before proceeding with experiments in wireless telephony we will consider for a moment the wonderful properties of the element selenium.

We owe the discovery of this remarkable substance to Berzelius. He first isolated it in 1817, from the refuse of a sulphuric acid

manufactory at Gripsholme, near Tahlum in Sweden. Though not an abundant metal, it enters into the composition of many minerals; in its chemical and physical properties it resembles sulphur and tellurium. Knox pointed out in 1837 the conductivity of molten selenium for electric currents. May found that the conductivity of selenium is considerably influenced by light. Ordinary fused or vitreous selenium is an exceedingly bad conductor, its resistance being about 3.8×10^{10} (forty thousand million) times that of copper.

In February, 1873, a letter from Mr. Willoughby Smith to Mr. Latimer Clark was read before the Society of Telegraph Engineers, in which was announced the remarkable discovery (made by T. E. Mayhew) that the element selenium is a very much better conductor in the light than in the dark, the conductivity increasing from 15 to 100 per cent. when the light of an ordinary gas burner falls on the metal.

On May the 18th, 1876, Prof. W. G. Adams and Mr. R. E. Day communicated to the Royal Society a paper containing the results of a year's experimenting with selenium. Numerous physicists as Draper, Sabine, Bell, Werner, Siemens, Tainter, Bidwell, Moser, Frittz, Mercadier, and others, have pursued investigations in the subject, most of them using a galvanometer for their experiments. Bell, however, was the first to use the telephone for detecting the alterations of resistance; and this led to the construction of the photophone. As selenium offers a very great resistance to passage of the electric current, only very thin layers can be inserted in the circuit when the telephone is to be heard, because the resistance of the coils in the telephone has to correspond more or less closely with the resistance of the circuit.

The selenium surface has, however, to be large, in order that it may be as much as possible affected by the light. Bell and Tainter tried, therefore, to produce a selenium cell to answer that purpose—*i.e.*, they endeavoured to make a sensitive cell. After making about 50 different selenium cells, they succeeded in making their now well-known cell, which has a resistance in the dark of 1,200 ohms, and only 600 in daylight.

Mercadier soon followed with his form of selenium cell, which also proved itself to be very sensitive; but the most successful selenium cells are those devised by Mr. Bidwell, and those used here this evening are made on his plan. In fact, most of the

successful cells used at the present day are constructed on the plan originally communicated by Mr. Bidwell to 'Nature.'

When selenium is carefully annealed, by keeping it for some hours at a temperature of about 217° C, just below its fusing point, and subsequent slow cooling, it assumes a crystalline condition in which state its electric resistance is considerably reduced; and in this condition especially is it sensitive to light.

In selenium we have thus a substance which when in the crystalline condition will transform variations of light into variations of electrical resistance. It acts in such a manner that when light falls on it, its resistance is reduced in a proportion to

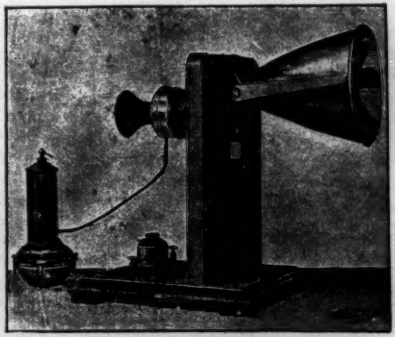


FIG. 1.—RUHMER'S LABORATORY TRANSMITTER.

the amount of light impingent. Adams found that the greenish-yellow rays were the most effective.

I will shortly perform a few experiments with selenium cells, to prove that they are reliable and sensitive, and I hope by that means to redeem them from the wrong impression which is held by many in this country, even scientific men, that they are unreliable and erratic. I can assure you they do not deserve this condemnation. I admit there are selenium cells sold which are not very sensitive; but that is simply the fault of the maker of the cells.

I will use in my present experiments a cell I constructed some time ago which I find very sensitive. I also have another here made by Von Bronk, and another by Ruhmer similar in construction to the cells used in his long distance wireless telephony experiments, in which he succeeded in covering a distance of 15 kilometers. Mr. Giltay, of Delft, is also justly famous for his sensitive cells.

At the opposite end of this room I have a complete "receiving station" for wireless telephony, which consists of a selenium cell, a condensing lens to collect and concentrate the undulating rays on to the selenium surface, a high tension battery, two dry cells in series with a relay, a bell and a telephone. It is so constructed

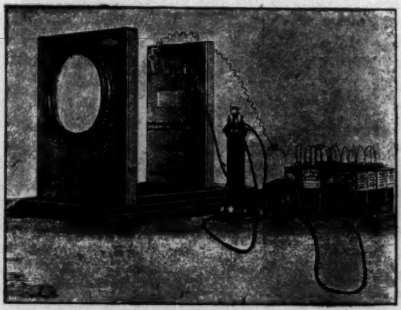


FIG. 2.—PROF. SIEMENS' LABORATORY RECEIVER.

that when the first ray of light affects the selenium, it causes the relay to close the bell circuit and ring the bell.

In order to draw attention, as in the ordinary telephone, on lifting the receiver from its support the relay and bell is cut out, and by holding the telephone receiver to the ear one hears the message transmitted from a distance as if it came over wires in the usual way; but only one can hear the message, and the words emitted from the microphone can be heard at the other end of this room even without any apparatus.

I might easily deceive you, and then it would be wireless telephony of another kind than the one I wish to demonstrate to you this evening; but in order to get over this difficulty I have this cell and battery connected with two wires leading to a telephone which is in a room where we cannot hear the words pronounced in front of the transmitter, and what is more, I will take the opportunity at the same time to show that a speaking arc lamp is not even necessary for our purposes, in fact, any kind of light would answer, provided it is sufficiently powerful and can be controlled by the human voice.

In this case I propose using an acetylene flame which is controlled by means of a telephone, as every word spoken into the microphone affects the diaphragm of the telephone in the usual way, and by carrying the gas through the telephone capsule before it reaches the burner, after the method of Koenig, all the vibrations of the diaphragm cause the elastic gas to be more or less compressed and, in turn, cause the burner to emit more or less light.

This undulating light affects the selenium, and the words are clearly reproduced in the telephone. Now, in order to be able to hold a conversation, I have also a microphone in the other room near the telephone, which microphone is connected to a loud speaking telephone in this room, so that the reply may be heard by everyone present. Perhaps the President will be so good as to speak before the transmitter which is here before us? May I ask Mr. Pharaoh to oblige us by listening to the telephone and responding from the other room; then we shall have a wireless conversation on a small scale.

Now you have heard the questions the President asked, and you have Mr. Pharaoh's reply; but it has not proved to you that it is light rays only which affect the selenium. Perhaps sound or other waves caused the selenium to be affected? This may be readily found out by placing between the selenium and the light various substances; at first I use a piece of glass—and Mr. Pharaoh replies; glass being transparent to light, it made very little difference to the selenium.

I now use a piece of iron—and we get no reply, because being opaque to light the cell was not affected.

I regret there is not sufficient time to mention the great number of other uses selenium could be put to. For instance, for the

self-lighting of the lamps in light buoys when it turns dark, and to extinguish them at daylight; or to extinguish the signal lamps on railways and steamboats. Could we make suitable sensitive cells of very low resistance, the greatest of electrical problems with which numerous inventors occupied themselves for a great number of years would be easier to solve. I am alluding to photo-telescopy, or seeing from one place to another at a great distance from it by means of electricity.

INTERIM REPORTS.

Presented November 20th, 1902.

Optical Glass Sub-Committee of the Optical Standards Committee.

This report contained the following recommendation: That, having regard to the importance of (*a*) a more definite specification of the optical properties of glass, a question intimately associated with the methods of producing the light used in the measurement of those properties, and (*b*) of a more definite method of recording the colour and actinic transparency of optical glass, the Sub-Committee recommends that it be authorised to proceed with the necessary investigations and to incur, if necessary, expenses not exceedng £10.

Spectacles and Eye-Glasses Sub-Committee of the Optical Standards Committee.

Recommendations Provisionally Adopted.

(1) **General.**—That the metric system be used for all linear measurements of frame or face; and that angular measurements be expressed by degrees.

(2) **Notation of Lenses.**—That the dioptric system be the standard for the measurement of spherical and cylindrical lenses.

(3) **Angle Notation for Cylindrical Lenses.**—For the right eye the numeration to be from 0° at the nasal side, proceeding upwards to 90° and then downwards to 180° on the temporal side, restarting from that point as from 0° and proceeding downward for the second semi-circle. For the left eye the numeration to be from 0° at the temporal side proceeding similarly to 180° on the nasal side, restarting from that point as from 0° and proceeding downward for the second semi-circle. Each division of the circle should be 5°.

(4) **Size of Eyes.**—Standard sizes to be based on the peripheral measurement of the lens; and it is suggested that the following would be a suitable series; size No. 1 having a peripheral measurement at 92.5 mm.; and each following size being larger than the size immediately preceding it by a number of millimeters equal to its own number in the series.

Following is a list of sizes 1 to 6 :

No. 1	length of periphery	92.5 mm.		
2	„	94.5 „	=	92.5 + 2
3	„	97.5 „	=	94.5 + 3
4	„	101.5 „	=	97.5 + 4
5	„	106.5 „	=	101.5 + 5
6	„	112.5 „	=	106.5 + 6

This numeration to be applied on the same basis of measurement to all shapes of eyes.

The recognised shapes are oval, long oval, round oval, half oval, round, pantoscopic.

(5) **Screws.**—Standard sizes of screws to be in accordance with those of the British Association.

Nos. 9 to 16 are recommended as the most suitable for spectacle and eyeglass work.

(6) **Spread of Bridge.**—Standard spreads of bridges to be segments of a curve, 1 centimeter in depth measured across at distances of 2 and 7 mm., from the apex of the curve. The spreads to be numbered from 1 to 6; No. 1 being the narrowest, and its chords to measure 6 and 9 mm. at the upper and lower distances respectively. The other spreads to increase the width progressively by 1.5 mm. at the upper, and 3 mm. at the lower chord. The dimensions of standard spreads would thus be as follows :—

No.	Upper chord.	Lower chord.
1	6 mm.	9 mm.
2	7.5 „	12 „
3	9 „	15 „
4	10.5 „	18 „
5	12 „	21 „
6	13.5 „	24 „

(7) **Eyeglasses Generally.**—Spectacle standards, in so far as they are applicable, are also recommended for eye-glasses.

(8) **Nomenclature.**—*Generic Terms*: Spectacles, eyeglasses, single eyeglass.

(9) *Parts of Spectacles.*—Frame, front, side, bridge, eye, joint, pin, screw.

(10) *Terms Applied to Eyewire*: Grooved wire (for a bevelled lens). Solid wire (for a grooved lens).

(11) *Qualities of Sides*: Curve, angle.

(12) *Names of Bridges (Spectacles)*: Crank or C, Arch or U, W, K, X.

(13) *Qualities of Bridges (Spectacles)*: Height, projection, spread, depth.

(14) *Classes of Eyeglasses*: Folding, non-folding, lorgnette.

(15) *Parts of Eyeglasses*: Bridge, eye, joint, screw, placquet, stud, handle, catch, stop, spur.

(16) **Methods of Measurement—Spectacles**—*Front:* The front of a frame to be measured from centre to centre of the pins. *Bridge*: The height of a bridge to be measured from the horizontal line bisecting the joints to the centre of the bearing surface of the bridge. The position above the horizontal line to be expressed in millimeters; the position below to be expressed in millimeters with a minus sign prefixed. The projection of a bridge to be measured from the plane of the back of the eyewire, to the centre of the bearing surface of the bridge. The position outward to be expressed in millimeters; the position inward to be expressed in millimeters with a minus sign prefixed. *Sides*: The length of a straight side to be measured from the inside of the joint to the end of the side when flattened on the rule. The curve of a side to be measured by the length of the sagitta. The angle of a side to be expressed by the number of degrees included by the side and the lower part of the eyewire. The dimensions of a curlside to be expressed by two measurements, viz. :—(1) The distance from the inside of the joint to a straight line tangent to the apex of the curve. (2) The full length, measured in the same manner as a straight side.

(17) **Eyeglasses**.—*Placquets*: The sub-committee has decided that it is desirable to record the inclination, and distance apart of placquets. The following method is suggested: A scale consisting of a central vertical line with other vertical lines on either side numbered in millimeters. The measurements of straight placquets to be taken at the top and bottom; of curved placquets at top, centre, and bottom; of rocking placquets, at the centre only. The length of the placquet should also be recorded if necessary. For non-symmetrical placquets the distance from the centre is noted for each placquet separately.

(18) **Height of Bridge**
(19) **Projection of Bridge**
(20) **Countersink of Side**
(21) **Length of Side**

These four subjects in the opinion of the sub-committee, are not suitable for standardisation.

ON SOME POINTS IN THE DESIGN OF OPTICAL INSTRUMENTS.

Paper by C. V. Drysdale, D.Sc., *Member of Council.*

Read before the Optical Society on December 18th, 1902.

ON being again invited to read a paper before a meeting of this Society, I was in great doubt as to what subject to select, and I finally adopted the above title not without considerable thought and many misgivings. When I read my paper on the Testing of Optical Instruments before you in the early part of this year, the subject was one of general interest, in that the retail Opticians were concerned in being able to ascertain the optical quality of the instruments coming into their hands. When, however, we come to consider the production of these instruments, we are met with the disagreeable fact that although this country is a large consumer of optical apparatus it is by no means a large producer, and further that as the demand for cheaper and better work has increased, our position as manufacturers has become relatively worse. It is, therefore, of the greatest importance that we should examine into this state of affairs and see how they may be improved, and it is greatly to be hoped that both manufacturers and retail Opticians alike will co-operate in doing all in their power to bring about this improvement.

Two great factors enter into the successful manufacturing of optical instruments as with other commodities. One of these is the manufacturing methods adopted, the other is the design of the optical and mechanical details. It is no use being able to turn out mechanical work very thoroughly indeed if you cannot use the proper lenses in order to produce images which are very perfect in definition; and I am afraid that is a subject which has very sadly been left alone in this country.

We stand, practically, as far as the theory of technical optics is concerned, where we did 70 years ago. In both of these ways we have been seriously falling behind of recent years, but in the former we are now keenly alive to the matter; and there is no doubt that with improved machinery and workshop processes much lost ground may be regained. (Hear, hear.) In the mechanical design of our optical instruments, too, we are not much behindhand, in fact, in certain respects English mechanical design is probably superior to all others. When, however, we turn to the optical performance of our instruments we are forced to admit that in almost all directions German instruments are in every way superior, and hardly a month passes without some improvement in telescopes, photographic lenses, or microscope

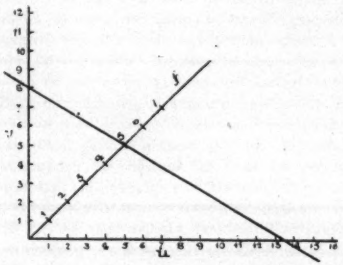


FIG. 1.

objectives emanating from the Continent, while but little advance is being made in England.

What is the reason of this? If we turn to the records of English work in the domain of technical optical theory we find that scarcely any progress has been made since the appearance of Coddington's "Optics" in 1829, and the first achromatic combination was produced by Dollond. Not one book of importance has been produced during this time until the translation of Professor Lummer's "Photographic Optics" was made by Professor S. P. Thompson in 1900. Not one of our English professors has made a special study of this subject. On the Continent we find that the principle of the correction of spherical

aberration was first enunciated by Euler, that of the correction of coma by Frauenhofer, and extended by Abbé, that of the employment of the aplanatic points of a sphere by Amici, while the rules for eliminating the errors of astigmatism, curvature, and distortion, were respectively put forward by Rudolph, Petzval, and Lummer. The conditions for flattening the field are indeed given in Coddington, but it remained for Abbé and Schott to make them practicable by the production of the anomalous Jena glasses. In addition we have the names of Steinheil, Von Seidel, Pulfrich, Czapski, Kaempfer, and many others who have distinguished themselves in this field. With such a catalogue of names and achievements it is little wonder that the science and

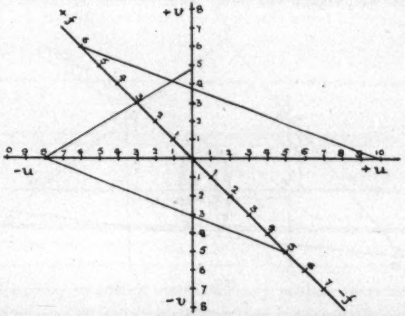


FIG. 2.

art of optics has made such strides on the Continent as almost to eclipse the work of British manufacturers, and it is only to be hoped that we may recognise as strongly as possible our backwardness in these directions, and that we may take steps to remedy it.

My apology, therefore, for bringing before you this evening a paper on such an intricate matter, must be the importance of the subject, and my reason for doing it threefold. In the first place I hope to be able to strip the subject of some of its intricacies, and to make the leading principles of design intelligible to those who have only an elementary knowledge of optics; in the

second place, to indicate a few new methods of surmounting these difficulties, and thirdly, and most important, with the object of pleading with our few scientific manufacturers that they should come forward and enlighten us as to their methods, as their continental contemporaries have so freely done. And in most cases they have done so not only for the benefit of their own country but for the benefit of the world. I do not mean to imply altogether that we should slavishly copy the German methods, but that we should seriously study the subject. Only by papers and discussions on this important subject can we regain our place, and I hope that the spirit of co-operation will induce us to lay aside all petty trade secrets and jealousies for the common good.

I have emphasised this latter point for another reason. When

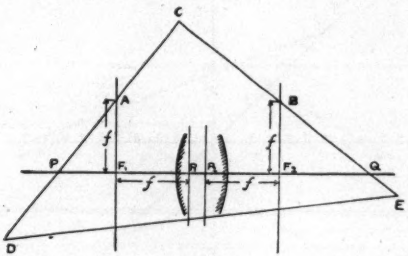


FIG. 3.

one comes to study the work of German scientific men in this or any other direction, one is immediately impressed with the immense amount of abstract mathematical or other scientific reasoning which there appears, and the colossal amount of what can only be described as intellectual drudgery they seem willing to undertake. In no subject is this more strongly marked than in that of technical optics, and I fear that if advance in this direction is only to be obtained by slavishly following the mathematical and numerical computations adopted by the Germans, we are not likely to rapidly catch up. Only those who have attempted it can realise the labour involved in the designing of such an instrument as a photographic lens, the hours and days of pure numerical computation required, which may have in

the end to be considerably modified by changes in the quality of the glass, or a slip in the calculation. No wonder, therefore, that English Opticians have very commonly an extreme contempt for theoretical design, and have preferred to obtain their results almost by pure experiment; and even at the present time there are a great many who, while admitting that the highest results can only be obtained by scientific methods, are yet doubtful as to the commercial practicability of adopting them. It is characteristic of the Briton that he prefers to get his results by direct short cuts, while his German cousin uses steady perseverance; and while we study and pay the greatest respect to the grand optical work of the Germans, there is no harm in trying if we can to infuse some British methods into them. For my own part, I feel certain that it can be done with great advantage.

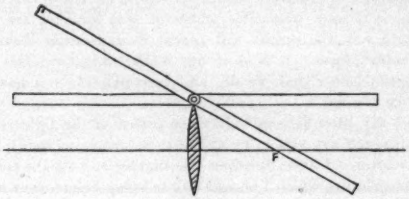


FIG. 4.

In designing an optical system for any purpose there are two main stages. The first is to arrange our lenses, prisms, etc., so as to get a system of a certain focal length, giving an image of the proper magnification, and sufficient illumination and field of view. The second stage is to consider the errors of aberrations of such a system and to settle the curves and glasses so as to eliminate these errors.

PART I.

In settling the lenses, etc., of any optical system, it is, of course, necessary for us to know how they modify the course of the light passing through them. There are three ways of doing this, two of which may be distinguished as geometrical methods, while the third may be termed the curvature or physical method.

In the first of these the course of the light is specified by the positions of the conjugate foci, and the lateral distances of the intersections of conjugate rays with reflecting and refracting surfaces. This may be termed the ordinary or text-book method. The second is due to Von Seidel, who departed from the ordinary method in substituting the angles of inclination of conjugate rays to the axis for the distances. The third or curvature method is the legitimate extension of the system used by Opticians, and consists in following the alteration of the convergence of the light through the system. Of these the first is undoubtedly the worst, the second has led to the magnificent generalisations of Von Seidel, while the third is comparatively untried. In my opinion, however, we shall find that this method will do much to simplify all optical processes even in the calculation of aberrations, and I have ventured to illustrate this in the second section of this paper. In fact, it is most noticeable whenever lens formulæ are being dealt with how the results will persist in expressing themselves in curvature form. It is as if our mathematical symbols knew their optics better than we do, and keep pointing out how they should be treated.

I need say little here as to how the course of the light may be traced through any system by any of these means, as it is a fairly simple matter. I have, however, one suggestion to make concerning nomenclature which I should like to bring before your notice.

It has been wisely decided to employ as our unit of curvature the diopter, that having a radius of one meter. Now in optical instrument work we frequently have to deal with large curvatures and small distances, the latter being more conveniently expressed in centimeters or millimeters. Why should we not therefore apply our ordinary metric prefixes to the diopter, and use a table like this?

RADIUS OF CURVATURE.	CURVATURE.
1 kilometer ...	1 millidiopter = 1/1000th diopter
1 meter	1 diopter
1 decimeter ...	1 dekadiopter = 10 diopters.
1 centimeter ...	1 hectodiopter = 100 diopters
1 millimeter ...	1 kilodiopter = 1,000 diopters
1 micron	1 megadiopter = 1,000,000 diopters.

When designing with distances and radii in millimeters the corresponding reciprocals would be the curvatures in kilodiopters.

Graphical Methods.—The numerical calculation of the path of the light through a system is frequently tedious, and as to make it intelligible it is best always to draw the course of the rays; it is troublesome to have to change from the drawing board to calculation and *vice versâ* repeatedly. What would be most eminently desirable in the first stages of designing is some method by which the course of the rays could be indicated on a drawing without calculation, and I have recently given some little attention to this subject to see whether any convenient methods are available.

Sir Howard Grubb's Method.—Where the system to be examined is made up of any number of thin lenses, this method for obtaining the positions of the conjugate points is of considerable value, and an apparatus suitably made on this principle is of great use in lens designing. As is well known, Sir Howard Grubb's method consists in reading off the conjugate distances

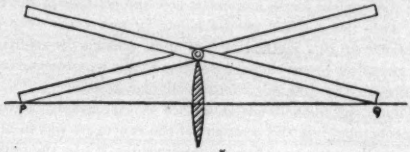


FIG. 5.

and the focal length on three scales, the scales for the two distances being set at right angles to one another, while the focal length is indicated on a scale at 45° to the other two. If a straight edge is employed, which is pivotted about a point which is set to the proper distance on the scale of focal length, then the intersection of this straight edge with the two axes give two conjugate positions, as is indicated in fig. 1, and however the edge is moved these pairs of conjugate distances can always be read off immediately. So far I have seen this arrangement described, it has been applied only to real foci produced by convergent lenses, but it is obviously extensible by producing all three scales as in fig. 2, so as to be used for real or virtual foci produced by either convergent or divergent lenses. It is possible with such an apparatus in a few seconds, therefore, to find the position conjugate to any given position with a lens of given

focal length, or to find the focal length required for any two conjugate distances.

I have, however, ventured to modify the method further, so that it may be employed by a draughtsman without reference to any separate apparatus, and to make it equally applicable to lenses of any thickness. All that is required for the purpose is a rule composed of two straight edges at right angles; in fact, for many purposes an ordinary set square will suffice. To make use of this method all that is necessary in finding the conjugate positions for any given lens is to measure off the focal length of the lens on both sides of it, as shown in fig. 3, and to then stick two pins, A and B, in the drawing board in these two focal planes, but at a height from the axis equal to the focal length. If then the set square, CDE, is placed with its two edges against the points A and B, the points where these edges intersect the axis will always be two conjugate points, and it is therefore possible on the drawing to see in a moment how the position of the image varies with the position of the object in any case. The great convenience of this method is that it is possible in addition to notice the effect of varying the position of the lens, or the separation between the different components of a system.

Although the method last described is exceedingly convenient and accurately gives the positions of the conjugate foci in accordance with the rules to a first approximation, it has the objection that it does not accurately indicate the course of the rays of light from the object to the image, and it can give no indication, therefore, of the conjugate positions for oblique pencils. I have, therefore, been led to devise two methods, one approximate, which is convenient for use with thin lenses; the other, which can be made as accurate as may be desired, and will give the course of the rays when refraction takes place at any refracting and reflecting surface. In the first of these all that is required is a jointed rule, preferably crossing like a pair of scissors, with two straight edges which can be fixed at any desired angle. In fig. 4 an application is shown of this rule to the finding of the position of an image formed by a simple convex lens of known focal length. In the case of a thin lens the refraction at any zone of it is, of course, equivalent to the case of refraction by a thin prism of definite angle, and we know that with a thin prism the deviation produced by it is to a first approximation indepen-

dent of the direction of the incident light. Consequently, if, as in the figure, we have any rays striking the lens at any distance from the centre, it is only necessary for us to stick a pin in the paper where the ray cuts the principal plane of the lens, and to place the rule so that the intersection of the two straight edges touches the pin. If, now, one edge of the rule is held parallel to the axis of the lens, while the other is bent to pass through the focal point, it clearly represents the path of a ray parallel to the axis being converged to the focus, and consequently gives the deviation produced by the particular zone of the lens. Secondly, if the rule is clamped with its edges at this angle, and it is then rotated about the pin, the two edges will always represent conjugate rays, and if one edge is made coincident with the incident ray, the emergent ray will be given by the other edge and can be drawn on the paper, fig. 5. Fig. 6 shows the diagram obtained

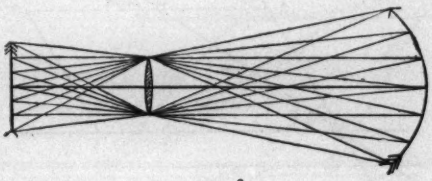


FIG. 6.

by applying such a rule to find the image produced by a straight line object at a distance from a convex lens, and indicates very strikingly the curvature of the image produced. Of course, it is well known that the curvature of the image surface is not definite, as we have three surfaces, one containing the primary focal lines, the other the secondary lines, and the third the circles of least confusion, but the directions of the rays of light are given with sufficient approximation to be useful for design purposes, provided that the angle of deviation is not large.

It may also be pointed out with advantage that this method is equally suitable to enable one to find the directions of conjugate rays in a lens of any thickness, provided that the curves are not very deep, and that the equivalent focal length is known. In this case, all that need be done is to stick two pins in the

paper instead of one, at the same height from the axes on the two principal planes of the lens, and if the rule is then held so that it touches both of these pins, the conjugate rays will be given as before.

An Accurate Method of Graphically Determining the Directions of Conjugate Rays.

Although the methods given above are of great utility in first scheming out a lens system, they can hardly be utilised when any more definite information concerning the lens system is required. It will be shown later that all the various errors to

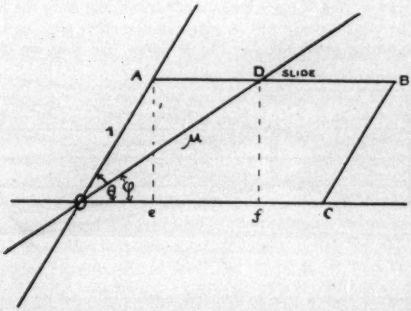


FIG. 7.

which reference has already been made depend upon the direction of the light and the distance from the axis at which the ray cuts the refracting surface. In order, therefore, to get a complete representation of the aberrations of a refracting system, as has been done by Von Seidel, it is necessary to follow mathematically the course of the light from surface to surface through the system, finding the refraction and the aberrations at each surface. The complexity of this process is extremely great, and it is here especially that the writer feels that some means of simplification ought to be attempted. If, therefore, it were possible accurately to determine the course of light through the lens

system without calculation, it would be possible to calculate the aberrations with very much greater ease.

Our fundamental formula for refraction is well known and may be stated in these terms, that whenever light is refracted on passing from one medium to another, the product of the refractive index of the medium and the angle which the light makes with the normal to the surface remains constant (this product divided by the radius of the surface is commonly known as Abbé's optical invariant). Consequently, if we were able to make any form of instrument or rule in which this law is fulfilled we should be able to get the directions of conjugate rays to any required degree of accuracy, and the attainment of this is not difficult. In fig. 7, OABC represents a frame jointed at all

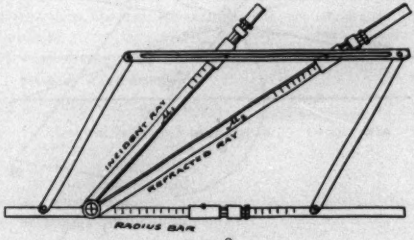


FIG. 8.

four angles, so that AB is always parallel to OC. OD represents another rod hinged at O and having at the point D a pin which slides along AB. The distance OD is made μ times OA. Then if OC is the normal to any refracting surface and OA is set in the direction of any incident ray, OD will be the corresponding emergent ray. For if we consider the perpendicular distances Ae and Df we know that $\sin\theta = \frac{Ae}{OA}$ and $\sin\gamma = \frac{Df}{OD}$ $\therefore \frac{\sin\theta}{\sin\gamma} = \frac{OD}{OA}$ since $Ae = Df$, hence $\frac{\sin\theta}{\sin\gamma} = \mu$.

Fig. 8 represents a rule designed on these principles, applicable to finding the refraction at any surface between any two media.

Two hinged bars are shown, each of which has a sliding point which can be set to any desired refractive index. Fig. 9 shows the application of the simpler form of instrument to find the conjugate rays in an actual case. Of course, if divided circles were attached, the angles of incidence and refraction, and of inclination to the axis, could be directly read off, to a considerable degree of accuracy. In my opinion such a rule, if well constructed, would give us the direction of the light with sufficiently great accuracy for it only to be necessary for us to calculate the aberrations at each refraction.

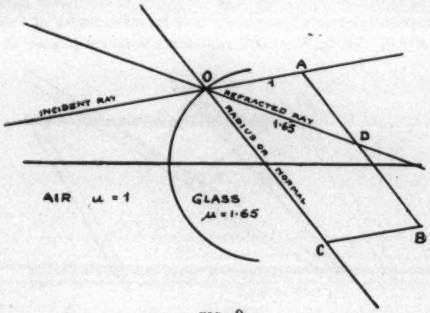


FIG. 9.

Part II.—Aberrations and their Correction.

We now come to the part of our subject which is of a more difficult nature, and can only be attacked completely by somewhat intricate mathematical processes. Nevertheless, the subject is full of interest, both from the scientific and commercial standpoint, and although it would be manifestly impossible to deal with it thoroughly in one or even several papers, I have ventured to write the following in the hope that it may interest some sufficiently to pursue it further, and that it may evoke scientific discussion.

In the first place, it will be well if we state scientifically what is meant by an aberration. If we make use of the ordinary approximate formulæ for lenses and mirrors, we find that to every point of the object corresponds a *point* in the image;

further, that if the object lies in a plane perpendicular to the axis, the image lies in a similar plane, and finally, that the image is exactly similar to the object. When an image is formed from an object in this manner, the two are said to be in *collinear* relationship, and this relationship is satisfied by the approximate or Gauss formulæ. When, however, we come to examine actual lenses more closely, we find that the conditions for collinear relationship are by no means satisfied, in that the light emanating from a point in the object is not focused to a point in the image, that the best image of a plane object lies on a curved surface, and that there is a certain departure from exact similarity between image and object. The deviations of the actual paths of the light from those which they should follow according to the approximation formulæ of Gauss are their aberrations, and it is convenient to classify these aberrations according to the effect they produce on the image.

This classification may be given as follows :—

Primary Aberrations :

Chromatic aberration. { Position. / Magnification.
Central spherical aberration.
Coma.
Radial astigmatism.
Curvature of field.
Distortion.

Secondary Aberrations :

Secondary chromatic aberration.
Chromatic differences of spherical aberration.
Chromatic differences of other aberrations.

We shall not refer to the secondary aberrations, but a little attention will be given to each of the primary aberrations and their correction.

Chromatic Aberration.—The general principles of this aberration can be found in many text books, but it is much more simply dealt with by the curvature or convergence method. Taking first the case of a simple thin lens, we have $F = (\mu - 1) C$ where C is the total curvature of the lens and F its convergence. Consequently, if we consider light of any two different colours, we have $F_1 = (\mu_1 - 1) C$ and $F_2 = (\mu_2 - 1) C$ and subtracting $F_2 - F_1 = (\mu_2 - \mu_1) C$ or $\triangle F = C \triangle \mu$ where $\triangle F$ is the difference of the convergences of the lens for the two colours, and $\triangle \mu$ is the dispersion or difference between the refractive indices of the glass

for those colours. The quantity $\triangle$ F I have ventured to call the chromatic convergence of the lens, and we therefore see that the chromatic convergence of a lens is equal to the product of its curvature and the dispersion of the glass.

But from the first equation, $C = \frac{F}{\mu - 1}$ and hence $\triangle F = C \triangle \mu = \frac{F}{\mu - 1} \triangle \mu = \frac{F}{\frac{\mu - 1}{\triangle \mu}} = \frac{F}{\nu}$ where $\nu = \frac{\mu - 1}{\triangle \mu}$

and is called variously the "achromatic refractivity" or the "efficiency" of the glass for the two colours concerned. In the catalogues of optical glass, these efficiencies are given, and consequently the chromatic convergence of any lens is given by simply dividing its convergence by the efficiency of the glass of which it is composed.

Nothing is easier when this result has been obtained than to find the chromatic convergence of any number of thin lenses in contact and thus to calculate an achromatic combination. For two lenses having convergences of F_1 and F_2 in contact, we have $F_1 + F_2 = F$ the combined convergence, and $\frac{F_1}{\nu_1} + \frac{F_2}{\nu_2} = \triangle F$ the combined chromatic convergence. If the combination is to be achromatic $\triangle F = 0$, and hence $\frac{F_1}{\nu_1} + \frac{F_2}{\nu_2} = 0$, or $F_2 = -\frac{\nu_2}{\nu_1} F_1$.

Hence, we have $F_1 = \frac{\nu_1}{\nu_1 - \nu_2} F$ and $F_2 = -\frac{\nu_2}{\nu_1 - \nu_2} F$ which gives us in a moment the convergences of the component lenses required to produce an achromatic combination of any desired convergence.

For example, if we require an achromatic combination of 20 cm. focal length of the following glasses :—

Chance's Hard Crown A. 605, $\mu_D = 1.5175$, $\nu_1 = 60.5$.

Dense Flint A. 361, $\mu_D = 1.6214$, $\nu_2 = 36.1$.

Here the focal length of the combination is 20 cm. and its convergence $F = +5$ D and $\nu_1 - \nu_2 = 24.4$.

Hence, $F_1 = \frac{60.5}{24.4} \times 5 = +12.4$ D, $C_1 = \frac{12.4}{.5175} = +23.97$ D, and $F_2 = -\frac{36.1}{24.4} \times 5 = -7.4$ D, $C_2 = -\frac{7.4}{.6214} = -11.9$ D. We therefore see that the combination must be made of a + 12.4 D crown lens, having a total curvature of 23.97 D, and a — 7.4 D flint lens with a total curvature of — 11.9 D. The actual face curves will depend on whether or not the combination is to be cemented, and other aberrations are to be corrected.

It should be borne in mind that it is frequently desirable to under correct or over correct the combination by a certain amount to compensate for an eyepiece or other lens. This can be done immediately by putting $\frac{F_1}{\nu_1} + \frac{F_2}{\nu_2} = \triangle F =$ the required chromatic convergence, and we then have $F_1 = \frac{\nu_1}{\nu_1 - \nu_2} F + \frac{\nu_1 \nu_2}{\nu_1 - \nu_2} \triangle F$ where $\triangle F$ is the amount of over correction required.

In photographic work, as is well known, the efficiencies of the glass should be taken from the D to the G lines of the spectrum, instead of from the C to the F lines, as given in the catalogues. These efficiencies can be calculated from the data given in the lists.

Chromatic Aberration of Lens Systems.—In the case of thin lenses the procedure given above enables us to completely correct a combination for chromatic aberration, with the exception of the small secondary spectrum which remains owing to the irrationality of the dispersion of the glass, and which can be reduced by the employment of suitable pairs of glasses. When, however, we come to lenses having a definite thickness, or to a combination of lenses which are separated by considerable distances, we find that chromatic aberration manifests itself in two ways, which are not simultaneously corrected as a rule. This will be understood in a moment if we consider the case of two thin lenses separated by a certain distance. If f_1 and f_2 are the focal lengths of the two lenses respectively, and d the distance between them, we know that the "back focus" or distance from the second lens to the focal

plane is equal to $\frac{f_2 (f_1 - d)}{f_1 + f_2 - d}$, while the equivalent focal length of the combination is $\frac{f_1 f_2}{f_1 + f_2 - d}$. If we make the first amount the same for different colours, we shall then find that the images of a distant object will all lie in the same focal plane, in other words, there will be no chromatic difference of position, but it will be found that if this is done it will not as a rule make the quantity $\frac{f_1 f_2}{f_1 + f_2 - d}$ the same for all colours, consequently the equivalent focal length will be different for the red and violet light, and the result will be that the image produced by one colour will be larger than that produced by the other. In this case we shall find that the image is perfectly colourless at the centre, but coloured at the margins, as is frequently the case; on the other hand, if we arrange that the equivalent focal length is the same for all colours it will be found, as a rule, that the images are formed in different focal planes. We are, therefore, led to distinguish between two principal kinds of chromatic aberration in lens systems, one the chromatic aberration of the position of the image, the other the chromatic aberration of the equivalent focal length or magnification. It would be impossible to go into this subject thoroughly without a fair amount of mathematical work, but it will be well to note that in the case of two thin lenses separated by a definite distance, which is a frequent combination for eye-pieces, we get the complete achromatism of the equivalent focal length by making both lenses of the same kind of glass and separating them by a distance equal to half the algebraic sum of their focal lengths, or $d = \frac{f_1 + f_2}{2}$. This is the case in the Huyghens or negative eyepiece, which is, therefore, used whenever possible for microscope and telescope work. This eye-piece is not chromatically corrected for differences in the positions of the images, but this can be got over in practice by slightly over correcting the object glass as indicated above.

Central Spherical Aberration.—The calculation of spherical aberration involves a certain amount of mathematical work, which

I do not propose to give here. It will be sufficient if I mention that the calculation may be considerably simplified by the introduction of the curvature or dioptric system, and give the results obtained.

As is well known, the effect of central spherical aberration is to make the lens with a convergence or divergence stronger at the edges than at the centre. We know that when light is refracted at a single spherical surface the ordinary formula is $\mu_2 V - \mu_1 U = (\mu_2 - \mu_1) R = F$ where U and V are the convergences of the light before and after refraction, R the curvature of the surface, and μ_1 and μ_2 the indices of refraction of the first and second media respectively. When we come to consider the effect of spherical aberration we find that the formula becomes $\mu_2 V - \mu_1 U = (\mu_2 - \mu_1) R + \frac{\mu_1 (\mu_2 - \mu_1)}{\mu_2^2} \left\{ (R - U)^2 (\mu_1 R - (\mu_2 + \mu_1) U) \right\} \frac{y^2}{2}$ which equals $F + a \frac{\lambda y^2}{2}$ where $a = \frac{\mu_1 (\mu_2 - \mu_1)}{\mu_2^2} \left\{ (R - U)^2 (\mu_1 R - (\mu_2 + \mu_1) U) \right\}$

This shows us, therefore, that the lens becomes stronger as we proceed from the centre to its edge, and that the increase of its convergence is proportional to the square of the distance y from the axis. The quantity a I have ventured to term the co-efficient of spherical aberration of the surface for the light considered.

If we consider the case of a thin lens of refractive index μ in air it is easily found that the co-efficient of spherical aberration a for the lens is $\frac{\mu - 1}{\mu^2} \left\{ (R - U)^2 [R - (\mu + 1) U] - (S - V)^2 [S - (\mu + 1) V] \right\}$ where R and S are the curvatures of the front and back faces of the lens respectively, and U and V the convergences of the light before and after passing through the lens. If the incident light is parallel, as is the case in telescope and photographic lenses, U = O and the formula then becomes $a = \frac{\mu - 1}{\mu^2} \left\{ R^3 - (S - F)^2 [S - (\mu + 1) F] \right.$

This formula may be used to find the spherical aberration of any lens with very little difficulty, but it is possible to obtain the spherical aberration of any lens with hardly any calculation whatever by having recourse to curves plotted from the results of this equation. If, for example, we take a + 1 D lens, we can calculate the co-efficient of spherical aberration for different values of the curvatures of one of the faces, say the back face, for glass of any particular refractive index, and on doing this we get the curve shown in fig. 10.

From this curve we see that if the back surface of the lens is plane or of no curvature the co-efficient of spherical aberration has a value of about 2.5 units, while with any other curve the

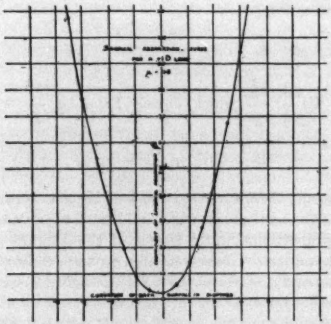


FIG. 10.

amount of spherical aberration is easily read off. If the lens has a convergence of any other value we have merely to multiply the value of a taken from the curve by the cube of the convergence in diopters, and we then obtain the co-efficient for the lens for parallel incident light. Similar curves may be plotted for different values of the refractive index, and of the convergence of the incident light, and I have found them most valuable.

Aplanatic Points of a Sphere.—A principle which is extremely valuable in the correction of spherical aberration of microscope objectives was first introduced by Amici, and is mentioned here as it is not called attention to in most of our optical text books.

Our formula above for the spherical aberration at any surface between the two media of any refractive indices shows that if R = U or μ_1 R = (μ_1 — μ_2) U the co-efficient of spherical aberration is zero, in other words, the spherical aberration vanishes. In the first case the light is, of course, incident directly on the spherical surface, and no refraction is produced, consequently the refraction has no optical value, but in the second case where U is equal to $\frac{\mu_1}{\mu_1 + \mu_2}$ R or $\mu = \frac{\mu_1 + \mu_2}{\mu_1} r$ we have refraction without any spherical aberration, and it will be shown later without coma; this is illustrated in fig. 11, which shows a sphere of glass and two points P and Q. If r is the diameter of the sphere, and μ is its refractive index, then if P is at a distance from the

FIG. 11.

centre of the sphere equal to $\frac{r}{\mu}$ and Q at a distance from the centre equal to μr, then the condition given above is satisfied, and every ray of light proceeding from P will appear after refraction at the sphere to come from Q, however wide angled the pencil may be. It will be seen, therefore, that if instead of a complete sphere a hemisphere of glass is used, as in fig. 12, and the object is in front of the hemisphere at the correct distance, while between it and the hemisphere a liquid of the same refractive index as the glass is interposed, we shall have a perfect virtual image of the object at the point Q. This image may be further magnified by having a second lens behind the first, as shown, having the

centre of curvature of its first face at the point Q, and its second face such that Q is one of its aplanatic points, but it is not possible to form a real image in this way. In order to form a real image it is then necessary to use other lenses which are usually cemented over corrected combinations to compensate for the chromatic aberration of the hemisphere. This is, of course, the principle of the construction of immersion microscope objectives.

Coma.—If the errors of chromatic and central spherical aberration are corrected, the image of an object on the axis is a perfect point. When, however, we have light emanating from a point on one side of the axis, the light meets the lens obliquely, and we then have a new series of errors of which the first and most important is coma. The effect of coma can be easily understood

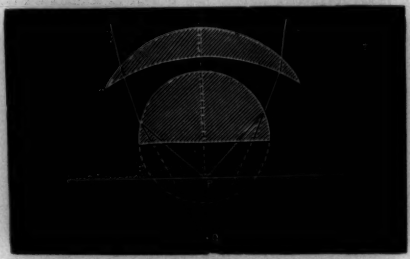


FIG. 12.

by reference to fig. 13. If we consider three rays from the object, one passing through the optical centre of the lens, while the other two pass through the upper and lower edges of it, it is obvious that in the case given the lower ray is refracted before the other, and consequently will intersect the central ray at a point closer to the lens than the other marginal ray does, the result will be that the light is not brought to a point focus, but to an unsymmetrical blur, from which the name coma is derived. Photographs of this defect were shown in my former paper. The effect of coma, however, depends, like that of spherical aberration, on the form of the lens, as will be seen on reference to figs. 13 and 14. As has been before said, the edge of the lens refracts like a prism, and

the deviation produced by the edge is consequently dependent on the direction in which the light meet the surfaces. In fig. 13, the lens is double convex, and it is easily seen that not only is the light at the bottom refracted to a nearer point on account of its meeting the lens earlier, but it also suffers a greater deviation as it meets the surfaces more obliquely, while the deviation of the other side is actually reduced. On the other hand, if the lens is made convexo-plane, as in fig. 14, it will be seen that the effect of obliquity is to make the deviation of the bottom ray smaller than that of the top, and hence to reduce the defect. In order to eliminate coma, therefore, our aim should be to construct the lens so that the effect of early refraction is compensated by diminished deviation, and this can be done with suitable lens combinations. It has been shown by Prof. Abbé, and more simply by Mr. Hockin, that the defect of coma is eliminated if what is called the *sine condition* is satisfied, *i.e.*, that the ratios of the sines of the angles

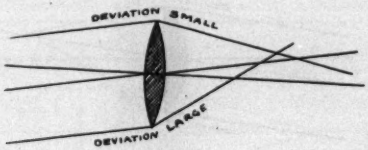


FIG. 13.

which the conjugate rays make with the axis are equal for points on the axis. Mr. Dennis Taylor has given a valuable method of correcting coma in the case of telescope objectives, and I have found that a combination of his formula with the curves for spherical aberration I have given above of considerable value. Coma is also eliminated in any system if the second of Von Seidel's conditions is complied with.

Radial Astigmatism.—This aberration is one which is easily understood with the aid of the wave or curvature theory, and is well known to Opticians in connection with tilted lenses. Figs. 15 and 16 show in plan and elevation what occurs when a plane wave of light is refracted at a tilted convergent lens, and it is seen immediately that the effect of tilting is two-fold. In the first place the light has to pass through a greater thickness of glass, and in the second place, in the meridian perpendicular to the

axis about which the lens is tilted, the lens acts as if it were of smaller aperture. Owing to these two causes, as is well known, the lens acts as a somewhat stronger spherical lens combined with a cylinder having its axis coincident with the axis of tilt.

If F is the convergence of the lens, μ its refractive index, and θ the angle through which it is turned, we find that the spherical convergence is increased by an amount equal to $\frac{F\theta^2}{2\mu}$, while the strength of the cylinder $= F\theta_2$. The strength of this cylinder is the radial astigmatism, and it consequently follows that the radial astigmatism of any lens in diopters may be simply obtained by multiplying its convergence in diopters by the square of the angle of obliquity in radians. As this does not appear to be generally known, and may be of value in visual work, I give the

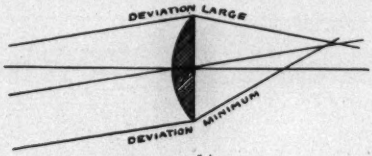


FIG. 14.

equivalent formula when θ is in degrees, we then find that the astigmatism in diopters equals $\frac{\theta^2}{3280}$ multiplied by the convergence of the lens in diopters.

It will be noticed that since this astigmatism only depends on the convergence of the lens, and not in any way upon its refractive index or face curves, it is impossible in any way to correct or reduce the astigmatism of any combination of thin lenses in contact. It has frequently been assumed, therefore, that radial astigmatism is incapable of correction, but Von Seidel has shown that this is not the case for separated lenses or systems having considerable thickness, and the astigmatism may be corrected by satisfying the third of his conditions.

Curvature of the Field.—If all the aberrations given up to the present are corrected, the result will be that a perfect image of

an extended object can be produced, but, as is well known, it will lie on a curved surface instead of a plane, the amount of this curvature may be very considerable, as indicated in fig. 6. When radial astigmatism is not corrected there are three surfaces, one containing all the primary focal lines, another all the secondary focal lines, while between them lies a third surface in which is contained all the circles of least confusion, which is the nearest approach we can get in this case to a perfect image. When astigmatism is completely corrected we have one surface only, corresponding to that of the circles of least confusion. The radii of curvature of the three surfaces are respectively $\frac{\mu}{\mu + 1} f$,

FIG. 15.

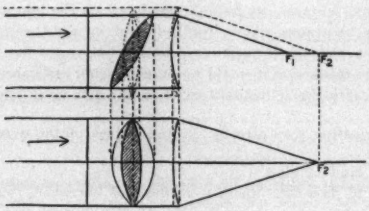


FIG. 16.

$\frac{\mu}{2\mu + 1} f$, and $\frac{\mu}{3\mu + 1} f$.

If radial astigmatism has already been corrected, and we have a system of any number of thin lenses, we can make the image lie in one plane if the sum of the convergences of the lenses each divided by its respective refractive index is zero. For example, if we have two thin lenses either in contact or separated we shall have a flat field if $\frac{F_1}{\mu_1} + \frac{F_2}{\mu_2} = 0$.

On referring to the section on chromatic aberration it will be seen that for two thin lenses in contact we have $F_1 + F_2 = F$

(convergence) and $\frac{F_1}{\nu_1} + \frac{F_2}{\nu_2} = 0$ (achromatism), and we now have $\frac{F_1}{\mu_1} + \frac{F_2}{\mu_2} = 0$ (flat field).

These three conditions can only be satisfied at the same time if $\frac{\nu_1}{\mu_1} = \frac{\nu_2}{\mu_2}$ or if the efficiencies of the two glasses are proportional to their refractive indices. On reference to an ordinary table of optical glass it will be seen that as the refractive index of the glass increases the efficiency generally decreases, and until the anomalous Jena glasses were produced it was therefore impossible to produce such an achromatic combination of thin lenses in contact. Now, however, we have glasses in which the relation above given can be approximately fulfilled, and we are therefore able to produce what are termed new achromats having a flat field. I venture to think that it would be an additional assistance in lens design if optical glass manufacturers would give us, in addition to the information they already do, the values of the quotient $\frac{\nu}{\mu}$ for each kind of glass.

There is, however, another method of correcting curvature which has been introduced by Mr. Dennis Taylor and Messrs. Busch, and again recently by Dr. Steinheil, in which the use of anomalous glasses is avoided. The condition for flat field is as given above $\frac{F_1}{\mu_1} + \frac{F_2}{\mu_2} = 0$. If both lenses are of the same glass $F_1 = -F_2$ and the lenses neutralise when in contact. On separating them, however, the combination now has a definite convergence with a flat field, and by extending this principle to three or more lenses, other aberrations may be corrected.

Distortion.—The question of distortion can be dismissed in a few words, as the defect is well known and its remedy in the case of photographic lenses also fairly well understood. It seems curious, however, that the general principle of its compensation does not seem to have been recognised until Lummer brought it forward in the year 1896, and it was independently found by

myself and given in my lectures some four years ago. In order that the image should bear exact similarity to the object only one condition is required, that the chief rays proceeding from each point in the object to the diaphragm should, after refraction, emerge from a point on the axis, in other words, that the lens system should be aplanatic with respect to its entrance and exit pupils. When this is the case the image is formed precisely as in a pin hole camera, and there can subsequently be no distortion. It is therefore necessary to eliminate distortion to correct the spherical aberration, not only from positions of the object and image, but for the points at which the images of the diaphragm are formed. As is well known, and as is indicated by the formulæ and curves given above, spherical aberration is different for variations of the conjugate positions.

With this summary of the aberrations and indications as to their removal, I must bring this somewhat lengthy and imperfect paper to a conclusion. As I stated at the outset, my object has been rather to attempt to interest English Opticians in this important subject and to show that it is not so extremely intricate. In my opinion an immense assistance would be given to designers if tables and curves similar to those given in the paper were calculated, and I should like to see some members of the Optical Society say, or others who took an interest in these things, get together and calculate out for us a set of tables, somewhat like Dr. Thompson's book of optical tables, but which only refers to elementary matters. I want to see spherical aberration, and other aberrations, set out, just as we see nautical almanacks, &c., are got up; so that we could all refer to such a table, and get the spherical aberration of a lens, and compensate it without very much difficulty. It can be done with concerted and co-operative action. Mr. Salt asked for co-operative action in certain directions of the optical trade. May I ask that we should try and co-operate in pushing forward some of the higher branches of the optical industry. I think we should try to raise our status in that direction as well as in every other. (Cheers.)

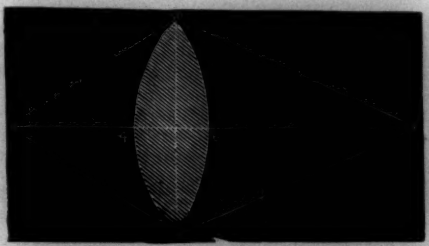
The Chairman: You have heard Dr. Drysdale's paper, and he has kindly invited you to comment on it; therefore, I should be exceedingly glad if anybody would do so.

Mr. Dixie: May I have the privilege of proposing a very cordial vote of thanks to Dr. Drysdale for the most interesting

and able paper which he has read us this evening? The whole paper bristles with interesting points, and there are a great number of questions which I should like to ask Dr. Drysdale, but really one does not know where to begin. The most interesting point, and that most present in my mind now, is a question that arose on the last diagram that was thrown on the screen. I assume that that curve was calculated for parallel light. What would be the effect if other sources of light were used? That is one thing. Another thing, perhaps, might be worth mentioning: On behalf of working Opticians, in a humble way, I desire to express my great sense of gratification that a scientific gentleman like Dr. Drysdale has bent his mind to the study of expressing mathematical formulæ based on the dioptric system instead of the focal length system, or whatever the old system may be called. I am convinced myself, without professing to any special knowledge on the point—it seems to me much more rational altogether to base your formulæ in your calculations on the first quality of the lens; and I am pretty well convinced that just as we have proved it of great advantage in our spectacle work—we have been practically driven to it—so I have no doubt that it will be in the construction of the more intricate instruments. I have no doubt it is the duty of us practical men to give a certain amount of thought to this; but it is more the duty, I think, of gentlemen in the position of Dr. Drysdale, who, after all, have nothing to do but think, or very little else—(laughter),—to hammer out some definite system for us. With regard to the dioptric system,—I always like to be practical, and if you would not mind I would like to show you what I mean on the blackboard. Supposing we take the very simplest formula, what you begin to teach your elementary wooden-heads, and which is in almost the very first lecture you give them—the law of conjugate foci. I could show you how very simple it would be to teach that in simply adopting diopters. I have a lens here which we will say is + 10 diopters. We are in the habit of writing it like that—all spectacle men know exactly what that means,—but if you are talking to a class you have to talk about the focal lengths. Very well, now, you take your point here, $\frac{1}{4}$ m. from the centre of the lens. Then we know that in order to bring that light to where we see it, all the diopters of our lens which I write thus are used. What have we left? Four from 10 = 6; + 6 left. Therefore, if we want to find the

focus which is conjugate to that point, this is like teaching that six and four are 10. That entirely explains the matter for a thin lens—it entirely explains the principle of conjugate foci. Of course, it may be extended—you may take the thickness into account, or a simple lens system into account,—add the distance between the principal planes, and so on. From that you could develop a great deal of the more complicated lens system. Again I thank Dr. Drysdale very much for the lecture he has given us, and I ask you to allow me to propose a very cordial vote of thanks. (Cheers.)

Mr. Salt: Mr. Chairman and gentlemen, I do not intend for one moment to criticise Dr. Drysdale's paper, nor to offer any counter or other suggestions as to the question of these lenses; but I feel proud in being able to second Mr. Dixey's vote of thanks



to Dr. Drysdale, who has always been so very kind in giving us the benefit of his "thinking" powers, which Mr. Dixey spoke of; and I do sincerely join Mr. Dixey in offering the lecturer our thanks for giving us his paper to-night. (Cheers.)

Dr. Mullineux Walmsley: I had not the slightest indication of the line Dr. Drysdale was going to take in this paper, nor did I know at all what he was going to say. The first part of his paper was introductory, and was a very strong plea for an Opto-Technical Institute, if for nothing else. (Hear, hear.) In regard to Dr. Drysdale's remarks on that subject, we do know that modern methods of manufacture have gone ahead at a tremendous rate. At this late hour one cannot pretend to go over all the points Dr. Drysdale has raised. In regard to what he said about

computers, a British astronomer found the planet Neptune, a task quite equal to any optical problem you can set. British computers have arrived at a very perfect knowledge of the planets; but we have not yet been able to convince them that optical problems are worthy of their attention. Therefore I say we have the computers in the country; and if the optical trade can only convince them that these computations will be for the advancement of optical knowledge, so much the better. But far more important than questions of computation or dead level calculation is the question of the development of the photographic lens—a question which I venture to think Dr. Drysdale's paper has advanced very considerably. Those early methods of Grubb are old and well known—well known in one sense, but I doubt whether they are known to most teachers of optics, simple though they are. And the method of working in curvatures instead of in focal distances and linear distances, I should say is almost unknown ground, outside the optical trade. Personally, I have taught to all my elementary optical students during 15 or 16 years, working in curvatures instead of linear distances; but until I went to the Northampton Institute my students were never students in connection with the optical trade, and teaching the general science of optics as pure science, one never carried it far enough. But the ordinary student could quite as easily work with the curvatures (at least, that has been my experience), whereas if it is left until the time when the student becomes the practical Optician it is too overwhelming a subject for him to take it up at all. I think you will agree with me that we are very fortunate in having at the Northampton Institute one so competent to teach the science of optics as Dr. Drysdale. (Cheers.) I should like to say, if I may do so without any suspicion of boasting, that the first task of leading the optical student forth on scientific lines really fell, at the Northampton Institute, into Dr. Drysdale's hands; and I consider that was a very fortunate thing indeed. One of the greatest advantages in Dr. Drysdale's methods in regard to this matter, is the saving in the computation time. You may eventually have to make a final calculation, because the method is obviously only an approximation, but it is so very nearly accurate as to be sufficient for practical purposes. I may say for 15 or 16 years I have been dealing with these matters. You have only to look at the formulæ, commencing with 1 over this, that

and the other—1 over linear distance is curvature; whatever may appear in the formula, or whatever the symbol, if only linear distance is the denominator it represents a curvature in the system. Curvatures are the things for Opticians, and linear distances are only to be used by astronomers. As regards tables being calculated by the Society, we have heard of that being done by other societies, in other directions. I might venture to suggest that even more useful than tables would be outlines of curves, such as we saw on the screen. Cut out the template block, lay it down on your drawing paper on these outlines, and you get your approximate solution. It would take you a considerable amount of labour, of dead calculation, either by yourself or a deputy computer, in order to get such approximate results. Outlines of curves, I venture to think, would be far more useful to the optical trade than the diagrams relating to dynamo-electrical machinery, published in Germany, which are for the use of the electrical trade. (Cheers.)

The CHAIRMAN: The motion before you is that a very cordial vote of thanks be accorded to Dr. Drysdale for his paper. Will you kindly signify your approval, gentlemen, in the usual manner?

The vote of thanks was carried by acclamation.

DR. DRYSDALE: Gentlemen, I have to thank you very cordially for the kind way in which you have expressed your approval of the very slender information I gave you in my paper. It was only a very small portion of what I would have liked to give you in the time. At any rate, I hope the few remarks made may lead you to take more interest in the subject, if you have an opportunity of seeing the lecture in print. I do not think I have very much to reply to in the discussion. I thank those who have spoken for the kind remarks made about these methods. As far as Mr. Dixey's observations went, one point he raised has some interest—Whether the curves would be suitable for different convergences. I do not propose them to be used where time permits of a calculation; but it will be found in practice, if you are going to use a telescope combination, one of the lenses of the combination does have parallel light passing through it. It would be very easy for you to deal with such a case. If Mr. Dixey wishes it I can give him further information on that point. I should like, perhaps, to take a little exception to his remark that we have nothing to do

at the Northampton Institute but think out such things. I want to say that we have a very little time to think out such things; but perhaps that is not a very happy way of putting it, as we cannot get at these conclusions without thinking. The portion of our time that we can devote to optics is very slender, for the number of students we have in the optical department is, unfortunately, but a small porportion of the whole, and naturally most of our attention has to be given to the greater number. As things develop we hope that may not be the case. (Hear, hear.) I do not think I need say anything about the point raised bv Dr. Walmsley. I do not forget that we have excellent computers in the country; but the main difficulty is that the man who does the computation in optics must be a practical man also. You must have a man with a practical idea of what he wants to get before he will be useful to practical Opticians. Instead of a person who computes in a mechanical manner, when he has a formula adjusted, we want the combination of a scientific man who is able to use these calculations in a practical manner. We have got to do that if we are going to keep abreast of the times. As regards the curves, my suggestion is that the Optical Society should prepare a table of these things—that they should also give us the curves, which are of more value than the tables, but are not quite accurate enough for final calculation. At any rate, curves are of such enormous assistance that a collection of curves should follow the tables. I thank you very cordially for all the kind things you have said. (Cheers.)

ON THE BERTHONIAN SYSTEM OF STANDARDISING ASTRONOMICAL EYE-PIECES.

Paper by Mr. John James Hall, F.R.A.S.

Read before the Optical Society on January 15th, 1903.

IT affords me very great pleasure, though elsewhere under great pressure of work, to make my first acquaintance with your Society this evening, and to bring with me a contribution towards what I know you all have at heart—the standardisation of optical instruments.

It cannot be denied that the present state of things in this direction is most unsatisfactory. I do not, for a moment, infer that the difficulty of successful comparative observation lies entirely this way. Mr. Percival Lowell, in a remarkable paper appearing in the 'Monthly Notices' of the Royal Astronomical Society for November last, deals with a "Standard Scale for Telescopic Observation," but this has reference to the question of varying states of the atmosphere, and results depending thereon; a subject deserving of every consideration. But if, as he says, "each man realises this, but marks his own work on his own scale, as if he should take his own foot as the unit of length," surely it becomes us first to see that we agree among ourselves on points of relative instrumental detail; for what is the use or utility of much mathematical deduction depending upon instrumental observation if the instruments themselves are wanting in accuracy, or rather, if we are without some recognised and definite means of knowing their errors and *relative differences*, and making due allowances for the same? The 6-ft. focus of commerce alone, I contend, does not meet our requirements.

One of the last astronomical acts of my revered and honoured friend, the late Rev. Edward Lyon Berthon, M.A., F.R.A.S., of "Romsey Observatory" fame, but better known, perhaps, the world over, as the inventor of the "collapsible boat," was to devise a simple apparatus by means of which, in the quiet of the

study, by daylight or by lamplight, and without the aid of the telescope or even its object glass, an observer may assign a fixed value, or "standard," to each of his eye-pieces, which value or *constant* once arrived at and engraved thereon, is simply to be multiplied by the focal length of any particular telescope or, speaking more correctly, by that of its object glass, to give, at once, the power to one, two, or three places of decimals on that particular instrument.

I have now the honour of submitting the same to the members of this Society.*

The apparatus, here shown, consists essentially of three parts, viz.:

1. A stand of hard wood composed of a horizontal base and vertical pillar, placed near one edge, whereon, at a height of 10 ins., or thereabouts, from the said base, is placed a perforated bracket, or gallery, for supporting an eye-piece.

2. The well known Berthon *dynamometer*, which I prefer, now that this word is so largely used in a totally different direction, to call *dynameter*, preferably with dove-tailed sliding magnifier, and—

3. A set of three black discs on white cardboard, of 1, 2, and 3 ins. diameter, respectively, any one of which may be laid on the aforesaid base for examination.

The dynameter I show you is so well known as to need little description. The scale, on one side of a V-shaped opening, is so divided that each long division represents .01, or the 100th part of an inch, while these are each sub-divided into five parts, each equal to .002 or the 500th part of an inch. In use, by the system now under notice, it is laid on and carefully slid along the eye-piece until the inner edges are just in contact with the image of the black disc when the division opposite thereto will give its diameter in decimals of an inch.

Let me here proceed to quote Mr. Berthon's own words, as contained in one of the many charming letters which he addressed to me, the one in question bearing date "Romsey, November 5th, 1898."

"I send," said he, "for your acceptance my last contrivance for measuring the power of eye-pieces. I propose that all makers should adopt a uniform standard by marking on each E. P. the

* The only printed reference to this device appears in an obituary notice of the Rev. E. L. Berthon, M.A., F.R.A.S., in the M.N. Royal Astronomical Society, Vol. LX., p. 314.

power it gives on a distance of 10 ins., effected by using the little stand sent with this letter. Please mount it and (following the directions now given) place an eye-piece in the hole, and lay one of the cards (with the black disc on it) on the foot. The image will be seen above the E. P. and its diameter gauged by the dynamometer."

"This little image," to quote Mr. Berthon's remarks in another place, "is exceedingly well defined and bears magnifying; it is not within, nor actually close to, the eye-glass, but remains visible and unaltered in size at a considerable distance."

But to return to his letter first quoted. "Suppose," he says, "you take first the 1-in. disc, and an E. P. of very low power, and it is gauged at 11" (by which we must understand .11), "then divide 100 by that and you have 9.1, which may be called 'standard 9,' and the E. P. be so marked." In another letter Mr. Berthon says "100 or 1,000" is divided and the quotient is the standard number. In the one case, of couse, scale readings are taken to two, in the other to three places of decimals. But as the advantage is as 191 to 20 taking each complete .005 as .010, I have adopted 1,000 as the dividend all through in the table which accompanies this paper.

Table for Computing the Powers of Astronomical Eye-Pieces by Means of Berthon's Dynameter.

Dyna-meter.	Factor.	Dyna-meter.	Factor.	Dyna-meter.	Factor.	Dyna-meter.	Factor.
		.023	43.478	.037	26.316	.051	19.608
.010	100.000	.024	41.667	.038	27.027	.052	19,231
.011	90.909	.025	40.000	.039	25.641	.053	18.868
.012	83.332	.026	38.462	.040	25.000	.054	18.519
.013	76.923	.027	37.037	.041	24.390	.055	18.182
.014	71.429	.028	35.714	.042	23.810	.056	17.857
.015	66.667	.029	34.483	.043	23.256	.057	17.544
.016	62.500	.030	33.333	.044	22.727	.058	17.241
.017	58.824	.031	32.258	.045	22.222	.059	16.949
.018	55.556	.032	31.250	.046	21.739	.060	16 667
.019	52.632	.033	30.303	.047	21.277	.061	16.393
.020	50.000	.034	29.412	.048	20.833	.062	16.129
.021	47.619	.035	28.571	.049	20.408	.063	15.873
.022	45.455	.036	27.778	.050	20.000	.064	15.625

Dyna-meter.	Factor.	Dyna-meter.	Factor.	Dyna-meter.	Factor.	Dyna-meter.	Factor.
.065	15.385	.099	10.101	.133	7.519	.167	5.988
.066	15.152	.100	10.000	.134	7.463	.168	5.952
.067	14.925	.101	9.901	.135	7.407	.169	5.917
.068	14.706	.102	9.804	.136	7.353	.170	5.882
.069	14.493	.103	9.709	.137	7.299	.171	5.848
.070	14.286	.104	9.615	.138	7.246	.172	5.814
.071	14.085	.105	9.524	.139	7.194	.173	5.780
.072	13.889	.106	9.434	.140	7.143	.174	5.747
.073	13.699	.107	9.346	.141	7.092	.175	5.714
.074	13.514	.108	9.259	.142	7.042	.176	5.682
.075	13.333	.109	9.174	.143	6.993	.177	5.650
.076	13.158	.110	9.091	.144	6.944	.178	5.618
.077	12.987	.111	9.009	.145	6.897	.179	5.587
.078	12.821	.112	8.929	.146	6.849	.180	5.556
.079	12.658	.113	8.850	.147	6.803	.181	5.525
.080	12.500	.114	8.772	.148	6.757	.182	5.495
.081	12.346	.115	8.696	.149	6.711	.183	5.464
.082	12.195	.116	8.621	.150	6.667	.184	5.435
.083	12.048	.117	8.547	.151	6.623	.185	5.405
.084	11.905	.118	8.475	.152	6.579	.186	5.376
.085	11.765	.119	8.403	.153	6.536	.187	5.348
.086	11.628	.120	8.333	.154	6.494	.188	5.319
.087	11.494	.121	8.264	.155	6.452	.189	5 291
.088	11.364	.122	8.197	.156	6.410	.190	5.263
.089	11.236	.123	8.130	.157	6.369	.191	5.236
.090	11.111	.124	8.065	.158	6.329	.192	5.208
.091	10.989	.125	8.000	.159	6.289	.193	5.181
.092	10.870	.126	7.937	.160	6.250	.194	5.155
.093	10.753	.127	7.874	.161	6 211	.195	5.128
.094	10.638	.128	7.813	.162	6.173	.196	5.102
.095	10.526	.129	7.752	.163	6.135	.197	5.076
.096	10.417	.130	7.692	.164	6.098	.198	5.051
.097	10.309	.131	7.634	.165	6.061	.199	5.025
.098	10.204	.132	7.576	.166	6.024	.200	5.000

"To ascertain the power of any telescope," Mr. Berthon continues, "with E. P. 'standard 9' you multiply the focal length (in inches) by 9, removing the decimal point one place to the left."

"Suppose the focal length to be 42 ins., then,—

$$42 \times 9 = 37.8$$

"When measuring high powers the little image is so extremely small that it is difficult to gauge. In such a case use the 2-in. or

the 3-in. disc; the image will now be twice or three times as large, and the result must be multiplied by two or three according to the disc you use. This plan of measuring has the further advantage, when any two or all three discs can be gauged in succession, of taking the *mean of all the trials* and thus securing a correct result."

Let us look a little further into Mr. Berthon's meaning. Suppose in place of the 1-in. we take the 2-in. disc, the reading by scale being 22 (.22), then,—

$$100 \div 22 = 4.5, \text{ and } 4.5 \times 2 = 9.$$

Or, taking the 3-in., the reading by scale being 33 (.33), then,—

$$100 \div 33 = 3, \text{ and } 3 \times 3 = 9,$$

both as before. The beauty of the whole thing is at once apparent.

Personally I delight to do anything in honour of my friend's name and memory, and I am sure that the members of the Optical Society would wish to hear his own description *verbatim et literatim*.

But we may reverently suppose that the explanation was not intended to be exhaustive; it was presented only in a crude letter form. Indeed, I have no knowledge as to the extent to which Mr. Berthon proposed to carry it.

And here I may offer some remarks by way of showing the more practical application of the subject.

It is evident, in the first place, that the use of *decimals* in the "standard" number is a *sine quâ non;* for it will be seen that the same *integer* appears, over and over again, in a table, already referred to, which I have calculated and here append for use with this instrument. It contains a value for every division from .010 to .200 inch (191 in all) of the *dynameter* scale, together with the intermediate value of each unmarked *thousandth;* this being useful when either of two divisions is doubtful, or when a mean, say, of all three disc measurements happens to fall on such number. It is a pleasurable exercise, and one which I commend to the members of this Society. I thoroughly advocate the dual or triple mode of procedure when the image, as regards size, admits of it.

I may here incidentally mention that it has been suggested to me by a scientific relative* that red discs and a white scale with

* William Garnett, M.A., D.C.L., some time Demonstrator of Physics in the University of Cambridge, Principal of the Durham College of Science, and now of the Technical Education Department, L.C.C.

black divisions would perhaps be preferable to the black discs and brass scale, and so I think it would be in individual cases. Indeed, the Astronomer Royal and Mr. Arthur R. Hinks, M.A., of Cambridge Observatory, have only recently been discussing the desirability of dividing micrometer heads on *celluloid*. Of course, too, the system is adapted to other modes of measurement. in evidence of which I have myself used a *micrometer metal gauge*, though not more successfully.

I cannot, perhaps, lay claim to much experience with Mr. Berthon's apparatus. But a *bonâ fide* test, the only definite one to which I have subjected it, comes out in such a remarkably satisfactory manner as to inspire confidence in the system, while, I am bound to admit it, it does infinite credit to the Opticians who made me the eye-piece (a new one, specially to order) used in the experiment. This was stated to be a power of 400 on a 6-ft. focus, and the following is the result of my examination :—

Measurement of 3-in. disc by the Berthon *dynameter* } = .054 inch.

Corresponding factor, per table, 18.519,

$\therefore 18.519 \times 3 \times 72 = 4000.104,$

which, on removing the decimal point one place to the left (as Mr. Berthon pointed out), gives 400.0 exactly as the power in question —a highly satisfactory result. But it will be seen that *dynameter* readings of .018 on *one*-inch and .036 on *two*-inch disc give identical results.

In order to ensure concentricity with the eye-piece under examination the cards should be punched and made to drop over two steady pins slightly projecting from the base board, or an easier way would be, at least in point of manufacture, to make the cards circular instead of square, and to have *one* shallow steady pin right in the centre of the black disc, the pin itself being blackened. A fine screw motion to the *dynameter*, too, would be a great advantage. In either of its forms it might be made to slide between adjustable wooden or metal guides, while a fine screw, at either side of the gallery, brought into contact, each with its own end of the *dynameter*, would greatly facilitate and ensure accuracy—in disc measurement—far more so than is to be obtained by free hand motion.

It is an admitted fact, I suppose, that the powers of astronomical eye-pieces have been enormously over-rated. Is not a

partial explanation to be found in the present paper? Take the instance I have quoted, the 400. This was origially intended for an O.G. of 3.375-in. aperture, but the instrument has been rebuilt and fitted with a Steinheil O.G. of 3.080-in. (78 mm.), the focus of which I have not yet exactly determined, but I will call it provisionally, 45 ins. The 400 (on 6-ft. focus) becomes in my case :—

$$18.519 \times 3 \times 45 = 250.$$

If the instrument could bear it—I am simply speaking by way of argument—and I wanted a power of 400, I should have to order one of 635, approximately, on a 6-ft. focus; which is easily found by turning to the table of factors. The factor 29.412, which corresponds to a *dynameter* reading, on 3-in. disc, of .034, supplies the case, thus :—

$$29.412 \times 3 \times 45 = 397,$$

and

$$29.412 \times 3 \times 72 = 635,$$

thereby showing the extended use of the tables.

The following, based on a *dynameter* reading of .054 on *three*-inch disc, will be of interest. It shows at sight the effect of one and the same eye-piece on telescopes of different focal lngth.

No.	Focal Length.	Factor.	Multipliers.	Value in Inches.	Adopted Power.
1.	80	18.519	× 3 × 80	444.456	444
2.	72	18.519	× 3 × 72	400.010	400
3.	66	18.519	× 3 × 66	366.676	367
4.	60	18.519	× 3 × 60	333.342	333
5.	54	18.519	× 3 × 54	300.008	300
6.	50	18.519	× 3 × 50	277.785	278
7.	45	18.519	× 3 × 45	250.007	250
8.	42	18.519	× 3 × 42	233.339	233

The question of *relative least distance of distinct vision* between different observers is a matter which might fairly be discussed, although I regard it as of no great importance. The proper height of the bracket or eye-piece gallery, too, for E. P. focus ought to be agreed upon, and I look forward with some interest to the opinion of members on this as of the system generally. Mr. Berthon, I find, by marks on the vertical pillar, fixed the top of gallery at 9.75 ins. above the top of base board.

It only remains for me to say that the table which I herewith present is adapted to object glasses of almost all sizes and foci.

It has been calculated in duplicate and checked by the reversionary method. I heartily commend the system to constructors and users of telescopes generally, but specially, perhaps, to you, whose counsel and recommendations will be looked for, as members of the Optical Society.

Mr. Thornthwaite rose to propose a vote of thanks to the reader of the paper. He said that perhaps not all present were practically experienced in the use of Mr. Berthon's method. Personally he had employed it for a number of years. The dynamometer was practically a simple open wedge, divided on the edge; whereby a disc of light was measured. A very simple addition that did duty in place of a telescope was a long square box, perhaps 6 ft. in length, with a circular aperture of 3 in. or 6 in. diameter at its further end. On inserting the eye-piece in the closed end of this box, an optical image of the disc aperture was formed which could be seen by moving the eye some little distance from the eye-piece, and its diameter measured by the dynamometer. He had examined very many eye-pieces in this way, and quite agreed with the statement that the magnifying power was often found to be exaggerated. It was nearly 20 years since he (the speaker) was directly connected with the trade; but he had been associated with Mr. Berthon at that time, and was much interested in his clever inventions. There was a slight disadvantage attaching to this form of the dynamometer, as for optical purposes the edge of the wedge could not be made too thin; but a thicker form of instrument was preferable for measuring the thickness of round or square wire, or even of a sheet of paper, or practically anything of that sort. The dynamometer could be made by any man possessing a fair knowledge of mechanics, and graduated on any desired scale, as most Opticians had dividing engines. It was to be remembered that the reading did not give absolutely the largest diameter, but a slight chord. Their dear old friend Berthon had also designed an exceedingly valuable transit instrument having a prism in front of the object glass, which, when revolved, any desired object on the meridian could be brought into the field of view. He was an expert in the soldering of metals—such metals as cast iron and brass, which could only be soldered together with difficulty; and he was of a most kindly disposition, always ready to give assistance or information on the subjects interesting him. The

speaker had, to his regret, lost sight of Mr. Berthon during an interval of some years, but had met him at a meeting of the Royal Astronomical Society only a few weeks before his death. Unfortunately—like so many inventors, men of the most ingenious ideas—Mr. Berthon had, he believed, not made so much money out of his clever mechanical devices as we should have liked. He was rather a mechanician than a mathematician; but his work showed that he possessed a master mind.

The PRESIDENT seconded the vote of thanks that had been proposed by Mr. Thornthwaite, and said he felt sure that all would join in the encomiums that had been passed on Mr. Berthon. The lecturer, Mr. Hall himself, was a good astronomer, having built his own observatory in every detail, thus showing himself a true disciple of Berthon. He could assure Mr. Hall that the interesting paper which had been read would find a much larger audience than on the present occasion, when printed in the Transactions of the Society. The paper when in print would come before a very large body of Opticians indeed.

MR. HALL briefly replied, once more paying a tribute to the attainments of Mr. Berthon, whom he associated with another friend, his master in horology, the late Mr. James Padbury, of Bishop's Waltham; men whose "well done" he greatly missed; men who could never be replaced in his estimation.

HYGROMETRY.

PAPER BY MR. W. A. DIXEY, *Vice-President.*

Read before the Optical Society on February 19th, 1903.

THERE are three qualities of the air, its temperature, its pressure and its humidity, which are of interest to everyone, and which form a frequent subject of comment in our daily life and conversation; and to measure the temperature of the air we have the thermometer, and to measure its pressure we have the barometer, and to measure the humidity of the air we have the hygrometer. And while everybody has a thermometer and knows how to read it, and most people have a barometer and think they know how to use it, how few people outside a laboratory or a meteorological station own a hygrometer, or, if they had one, would know how to get any information from it; and this although in some respects the measurement of the humidity of the air is of more value and of more interest than the estimation of its temperature or pressure.

And I think there are two main reasons for the general neglect of the science of hygrometry; and the first is a want of knowledge of its principles, and the second is a lack of a simple, easily read instrument for measuring the degree of humidity. And as I suppose Opticians are chiefly to be blamed on both counts, I thought that would serve as an excuse for introducing to you to-night a subject which is not perhaps strictly optical.

So that the object I have in view in reading this paper is to try to raise your interest in the subject of hygrometry, and perhaps to indicate some simpler, and more generally acceptable, form of hygrometer than the instrument which we now usually associate with the name.

And now let us start at the beginning, and ask ourselves what hygrometry is.

Hygrometry may be defined as the measurement of the amount of water present in the air in the form of aqueous vapour.

We know that the air consists of a mixture of gases, chiefly

nitrogen and oxygen, in more or less constant relative quantities. And permeating this mixture of gases, to be regarded as in it but not of it, there is always a quantity of aqueous vapour, or water in its gaseous form.

It is one of the most familiar properties of air that it has the power of sucking up water, and absorbing it in the form of vapour. From the point of view of the water we call this evaporation.

The amount of vapour present in the air at any given time is very variable, and depends upon a variety of conditions. One of the tasks of hygrometry is to determine the exact amount. If we have an instrument which will show us the number of grammes of water in a cubic meter of air we arrive at what we may call the *absolute humidity* of the air.

But this is not the only task of hygrometry, nor indeed the most practical and most interesting. We have to realise what is meant by *relative humidity*. The air can only absorb a certain quantity of water; when it has reached this limit it is saturated, and can contain no more. If more vapour is projected into it when in this saturated condition, the extra vapour is at once deposited in the form of rain, or dew, or a film of water upon any convenient surface.

This limit at which air becomes saturated is called the *dew point*. Now it is important to notice that the position of the dew point, or the point of saturation, is dependent upon the temperature of the air. Hot air will contain a much larger quantity of vapour without becoming saturated, than will cold air. You may make a scale to compare with the scale of the thermometer of the maximum amount of water that the air will contain at each degree of temperature. Thus at

30 ° F the air is saturated when 1 cubic meter contains 4.5 grms.
40° „ „ „ „ „ 6.5 „
50° „ „ „ „ „ 9 „
60° „ „ „ „ „ 12 „

and so on.

So that supposing we have a cubic meter of air, and in that cubic meter of air 8 grms. of water in the form of vapour, at a temperature of 60° the air will be fairly dry, as it contains only 8/12 or 2/3 of the moisture which it can hold. But if the same air with the same amount of vapour is cooled down to 50°, it

will be very damp air, as at that temperature it contains 8/9 of its possible maximum. And if it is further cooled down to 40° it passes the dew point, and while 1.5 grms. (*i.e.*, 8-6.5) is deposited in the form of water, the air remains fully saturated.

So that here you see we have something different from absolute humidity. It is a relative humidity, and what we want in the instrument which is to measure for us the relative humidity of the air is a scale divided to 100 parts, of which the zero represents absolutely dry air, and the 100 completely saturated air. Such an instrument will tell us the percentage of saturation, or the relative humidity of the air.

Let us now consider what means we have for measuring the amount of water vapour in the air.

Perhaps the most obvious is the method of absorption. A given volume of air is made to pass over some substance which will entirely absorb the moisture in it. The difference in weight of the absorbent material before and after the experiment will tell us the weight of moisture that was in the given volume of air. In this way we get the absolute humidity.

If we reduce this to the number of grms. per cubic meter, and compare the result with the table of temperatures and maximum weights, we can read off at once the dew point; and the relative humidity is calculated from the formula $\frac{t \times 100}{t'}$ where t = the elastic force of vapour (which is the same as the weight of water in grms.) at the dew point, and t = the *e.f.v.* at the temperature of the air when the experiment was made.

The best absorbent to use in the experiment is strong sulphuric acid. A U tube may be filled with small pieces of pumice saturated with the acid. One arm should be connected, by means of an airtight fitted tube, with a large bottle filled with water. The tube passes airtight through the cork to the bottom of the bottle. Another tube bent as a syphon passes through the cork to the bottom of the bottle, and serves to draw out the water, and so maintain a constant flow of air through the U tube.

The experiment is one that is easily carried out in a physical or chemical laboratory, and affords, as I say, perhaps the simplest and most obvious means of measuring the amount of moisture in the air.

But it remains a laboratory experiment, and the principles upon which it is based do not lend themselves to the construction of an instrument which could be easily read or interpreted; and that is the chief requirement in an instrument which is to be used by the general public.

Another way of measuring the hygrometric state of the air is to find directly the position of the dew point. Daniell's, Dines's and Regnault's hygrometers have all this object, and I have here a modern form of Regnault's hygrometer which is the simplest and most practical instrument of this class that I have seen.

The physical law on which the instrument is based I have already alluded to, and common illustrations of the law fall within the observation of everybody. If you pour ice-cold water into a glass in a hot room, the surface of the glass quickly becomes covered with small beads of water. The reason of this is that the mass of cold water rapidly reduces the temperature of the layer of air surrounding the glass. This layer of air, which at its former temperature was capable of containing all the vapour with which it was charged, as it is cooled, reaches and passes the point of saturation; it can no longer contain the same amount of vapour, and the excess is deposited in the form of water on the surface of the glass. You will see that if this cooling process were conducted gradually, and if we had a thermometer in the glass of water, we could observe the exact temperature at which water began to be deposited, and this would give us the dew point.

That is exactly the principle of this instrument. Here is a metal cup with a polished surface on which we can easily observe the first appearance of the dew; into the cup is inserted a thermometer to mark the temperature; the glass screen is to guard the instrument from the breath of the observer; into the metal cup I pour a small quantity of ether, and by forcing air bubbles through the ether by means of the rubber bellows I cause the ether to evaporate, and so reduce its temperature. The thermometer marks my progress. I carefully watch the polished surface of the cup, and at the first appearance of the dew I note the temperature. This gives me the dew point; and from this I can, as before, calculate if I wish the relative and the absolute humidity of the air.

Here we have an instrument practical and useful enough in the hands of anyone who will take the trouble to understand it.

But still it does not reach our ideal of an easily read instrument suitable for the general public.

The chief point about this hygrometer is its accuracy. With careful observation, there is not much room for error, and in the good days when every member of the public shall possess his own hygrometer, this one should be kept by all Opticians for the purpose of testing and adjusting the popular instrument.

The next hygrometer which I have to describe is the so-called **Mason's**, or wet and dry bulb thermometer.

This hygrometer is the instrument which we, in England, generally associate with the name. It is based upon two physical laws:

1. That the rate of evaporation from a surface of water is inversely proportional to the percentage of saturation of the air in contact with it, and
2. That the act of evaporation entails an expenditure of heat proportionate to its rate.

The observation consists of reading the temperature of the dry thermometer, and the temperature of this other one, whose bulb is covered with muslin, which is kept constantly moist by means of a wick leading from a supply of water. If we call these two readings t and t' we can deduce the dew point from the following formula:

$$e = e' - .0009739\ t'\ (t - t') - .5941\ (t - t') \\ - .0008\ (t \,.\, t')\ (b - 755)$$

where e = elastic force of vapour.

e' = *e.f.v.* at t'

b = barometer height in millimeters,

and from the dew point we get as before the other hygrometric conditions.

These calculations have been made and are available to us in Glaisher's tables for all temperatures between 10° and 100° F. and for differences up to 24°. So that with the two thermometers and with a book of tables we can fairly easily arrive at the hygrometric conditions.

This is the instrument upon which we have mainly relied for the past 50 years to introduce the science of hygrometry to the general public; and that the result has been so poor is due, I contend, to the unsuitable nature of the instrument. When you have educated your average man to the point of reading a scale, as in a thermometer or barometer, you have pretty well reached

the limit of his capacity. To ask him to take two readings, and to compare them with a table of values, is to make a demand upon his powers of concentration altogether too severe.

But this difficulty of reading is not the only point against the Mason's hygrometer. To get correct results the instrument requires considerable care and attention; sources of error are many and fruitful.

1. The two thermometers must exactly correspond, not only at the points, but throughout the scale.
2. The muslin must cover the tube equally and entirely. A thick fold, a gap, a tear, dirt, or any other obstruction to the rapid absorption and evaporation of the water will interfere with the result.
3. The water must be absolutely pure.
4. The thermometer must be kept in a position where there is naturally or artificially a rapid current of air. This is most important, and most often overlooked. In stagnant air the layer of air surrounding the wet bulb becomes overladen with vapour, and the proper rate of evaporation is not maintained.

These possible defects, except perhaps 1, all tend the same way, *i.e.*, in the direction of lessening the difference between the two readings; so that, perhaps, after all, there is some excuse for the general public for having for so long looked askance at the Mason's hygrometer.

Where, then, are we to look for our popular hygrometer? You will recollect that I laid it down as one of the objects of this paper to indicate some simpler and more acceptable form of instrument than that which I have just described, and my suggestion is that we should revert to an older form of instrument which in this country, at any rate, we have practically lost sight of. It has long been known that human hair, properly selected and prepared, varies in length with the relative humidity of the air to which it is exposed. It has been found by experiment that when removed from dry to saturated air it increases consistently by 3/128 or 1/40 of its length. A hair hygrometer is no new instrument. I can recollect, when I was in Germany 25 years ago, seeing several instruments of this type in public places. It is described by Mr. R. H. Scott in the 1883 edition of his book on

"Meteorology," where he says: "I have described at some length the principle of the hair hygrometer, as although it is seldom used in this country it affords practically the most satisfactory means of determining the hygrometrical conditions of the air at temperatures close to and below the freezing point, under which circumstances the wet and dry bulb hygrometer fails." The chief advantage of the hair hygrometer, from my point of view, is that it allows of a scale from which can be read directly, by means of a pointer, the relative humidity of the air; which, for nearly all conceivable hygienic and domestic purposes, is what we want to know.

Its only drawback was its fragility. As I recollect the instrument, and as Mr. Scott describes it, it was constructed of a single hair. But this disadvantage has been effectually met in the instrument which I am showing you to-night. Here is a hair hygrometer made by Lambrecht in Göttingen, of a type which I believe is now widely used in Germany and Russia. Instead of the single hair we have here about twenty, with a result that the strength of the instrument is increased twenty fold.

There are other points in this hygrometer which evidence careful thought and great ingenuity. I would call your attention to the arrangements for adjusting it; the screw at the top corrects the position of the pointer (as we may set the hands of a watch). The nut at the bottom regulates the movement of the pointer relative to the scale (answering to the regulation of a watch). The scale on the dial gives the relative humidity, which you read off directly.

The thermometer is divided, as I have before suggested, with the degrees of temperature on one side, and the equivalent *e.f.v.* on the other. This enables you to find the actual vapour force present in the air by multiplying the amount of force shown on the right hand scale by the percentage of humidity shown on the dial, and dividing by 100. The actual vapour force being thus found, the dew point will be the degree of temperature opposite this reading. But in case this calculation should be too abstruse for the observer, a most ingenious device is provided by the constructor of the instrument. The number of degrees of difference between the dew point and the temperature of the air at different ratios of saturation is recorded upon an upper scale on the dial. So that to find the dew point it is only necessary to deduct the

number indicated upon the upper scale of the dial from the actual degree of temperature of the air. As this number of degrees of difference varies not only with the relative humidity, but also to a slight extent with the temperature, a further correction must be made, if an exact result is required; and to make this correction quite simple a further very ingenious device is adopted. You will notice that the end of the pointer is in the form of a trident, and on the scale at the base of the pointer is a similar trident with a figure 10 at its centre barb, a zero on the right, and a 20 on the left. According to the temperature you take your reading upon the centre right or left barb, or at an intermediate point.

I have ventured to demonstrate this instrument at some length, because of all the hygrometers I have recently seen, it seems to me to come closest to the ideal of an instrument adapted to the general public. Its chief virtue in this respect is the simplicity with which the relative humidity may be read; while at the same time other hygrometric conditions may easily be calculated by those who desire them.

I have left myself but little time to deal with the most important part of my paper—the application of hygrometry to practical purposes—and I shall not attempt on the present occasion to deal at all fully with the many ramifications of so extensive and complicated a subject. But I will ask you to bear with me a little longer while I try and indicate some of the more obvious uses to which hygrometric observations may be applied.

And I would group them roughly under three heads—

1. Hygienic.
2. Meteorological.
3. Technical.

And firstly with relation to hygiene; consider for a moment the effect of the hygrometric state of the air upon the physical condition of the body. From the skin a constant process of evaporation is taking place. The rate of this evaporation is entirely governed by the relative humidity of the air. Its effect upon the lungs is still more noticeable. With every inspiration nearly a pint of air is taken into the lungs; before its expulsion, this volume of air is heated and charged with vapour nearly to saturation point. So that a constant drying and cooling process is going on, for, of course, evaporation entails a loss of heat; and this process

again is entirely controlled by the hygrometric condition of the air, or, to be precise, by the position of the dew point.

We must all be conscious of the unpleasant effect of air that is too moist or too dry. Of the former variety we have sufficient experience in London. We call it by various names; we say it is heavy, muggy, oppressive, close. We vaguely attribute the unpleasant feelings it entails to the heat. But heat by itself is not unpleasant or injurious. I recollect some years ago a man telling me of an enjoyable cricket match played in Australia with a temperature of 123° F. in the shade. It is the combination of heat and moisture that is unpleasant and injurious.

On the other hand, air that is too dry is equally unhealthy. Our commonest experience of it is in an overheated room. Whatever there is of moisture in such a room is quickly dried up. The relative humidity falls, and the thirsty air attacks your skin and lungs. Evaporation is forced to its limit; notwithstanding the heat of the room, you feel cold; nervous irritability is induced, loss of appetite and insomnia.

Another experience of dry air is our old friend the east wind. Why does the east wind chill the marrow of your bones, penetrate the thickest clothing, and fill you with every variety of blue devils? Not because it is cold; a quite genial west wind is often much colder; but because it is dry. It desiccates your throat, it irritates your nerves, it dries and chills your lungs.

I should like to enlarge on the value of a hygrometer in choosing a house, in testing for dampness different rooms and cellars of a house, of the importance of hygrometric conditions in selecting a health resort, and especially of the use of hygrometry in the treatment of disease, and of the hygrometer in the sick-room. But time forbids; and I will conclude these desultory remarks on the hygienic aspect of hygrometry with these two rules of general application to healthy people.

1. The most favourable relative humidity lies between 60% and 75%. It should never fall below 40%, or rise above 80%.
2. A dew point higher than 20° C. or 65° F. should be avoided.

In its application to the science of meteorology, the hygrometer is again of considerable interest and value. Apart from the

study of climatic conditions, it is a great help in the difficult problems connected with weather forecasting.

I take the following rules from Prof. Hans Hartl's "Einführung in die Wetterkunde" :

1. A rising dew point points to a proximate rise in temperature, a falling dew point to a fall in temperature.
2. A rapidly rising dew point (about 6° C. in 12 hours) foretells a distant thunderstorm and a dew point of 17° C. or higher points to the imminence of a local storm.
3. Strong variations of the dew point foretell heavy wind.
4. If the dew point fall below freezing point a night frost may be expected.

Further indications of approaching weather may be gathered by comparing the morning temperature, taken at 8 a.m., with the position of the dew point at 6 p.m.

1. If the evening dew point stand higher than the morning temperature a storm is threatened.
2. If the evening dew point is not more than 4° C lower than the morning temperature rain may be expected.
3. If the evening dew point is from 4° to 8° lower than the morning temperature there is promise of fine weather.
4. If the evening dew point is 9° or more below the morning temperature wind and short showers are indicated.

For technical purposes in not a few industries the hygrometer has also many applications. In horticulture for the prediction of rain, storm, hail, early frosts, etc., as also for the proper regulation of the moisture in hot houses; in the treatment of cotton in looms; in the manufacture of tobacco; in brewing, and in many other industries where the success of processes is dependent upon the hygrometric condition of the air a reliable instrument is required.

I cannot on the present occasion attempt an examination of all its possible uses, and I shall conclude by again commending to your very careful consideration this specimen of a hair hygrometer, and by asking you whether you do not consider that an instrument made upon somewhat similar lines would be acceptable to the general public, and would prove a source of interest and benefit to its owner, and of profit and satisfaction to the Optician.

Dr. Drysdale said that although, as the lecturer had observed, the subject was in some respects an old one, it was of great interest. He would not hesitate to say that the instrument that had been particularly brought before their notice by Mr. Dixey came nearer to being a practical hygrometer, for the use of ordinary persons, than did any other. Many of those present were, no doubt, familiar with the usual instruments described in text books, but knew also of their imperfections. As Mr. Dixey had said, from the point of view of those practically interested in hygrometry the important thing was the relative humidity. Not one of the instruments to which he (the speaker) had referred just now gave this directly, and several of them were open to objection from a scientific point of view. With respect to the matter of adjustment, it was useless to expect the general public, or even people with a comparatively advanced knowledge of science, to do what the inventors and makers of instruments might themselves do readily. The corrections were made in a manner that was unique and very valuable, in the instrument brought forward by Mr. Dixey. Much might be added on the subject of the practical importance of hygrometry. For instance, the enormous influence that the condition of the air in this respect had on the cotton spinning industries might be mentioned. He believed that these industries were concentrated in the district surrounding Manchester, mainly because the hygrometric conditions were there favourable.

Mr. Thornthwaite said that the subject of hygrometry might be considered as, in a sense, closely connected with optics. He referred to the matter of optical definition. Everyone knew that if a speculum were exposed to cold, wet air, that would result in the deposition of a film of moisture upon it, which would prevent the attainment of optical definition. Under such circumstances, one must not bring the speculum into a warm room, to dry it off quickly, as that would cause damage to the silver film. While speaking of meteorology, he might refer to the recent lamented death of their old friend, Mr. Glaisher, a Fellow of the Royal Society, who was so very eminent a scientist, and whose son was equally distinguished in the special branch of mathematics. The life of the late Mr. Glaisher had illustrated the fact that the pursuit of science does not militate against longevity. His great adventures in the air, accompanied by the aeronaut,

Coxwell, would be within the knowledge of all present; and no doubt his painful experiences on his most memorable aerial voyage were connected not only with the coldness but also with the degree of humidity of the atmosphere. The speaker concluded by saying that he would like to be allowed to propose a hearty vote of thanks to Mr. Dixey for his most interesting paper.

Mr. Sutcliffe rose to second this vote of thanks. He remarked that what he was about to say upon the subject might be deemed hardly scientific. Mr. Dixey had pointed out the applications of hygrometry in ordinary life. Mr. Thornthwaite had indicated the importance of the matter in relation to optics. He (the speaker), when he first heard of this lecture, had looked up the technical term in question in the dictionary—a not unusual course with most people. Subsequently he had the pleasure of meeting the lecturer, who said that it was to his regret he had chosen "a fearfully dry subject." (Laughter.) Of course, added Mr. Sutcliffe, they would know what his answer to this was. (Laughter.) He had to confess that, until the lecture began, this comprised all that he knew about the subject. He was glad to be able to claim a better acquaintance with it now.

The vote of thanks having been put to the meeting, and carried *nem. con.*,

Mr. Dixey said that he was extremely obliged for the very kind way in which this vote had been put and received. Before adding a few further remarks, he would be glad to reply to any questions that might occur to anyone present.

A member said that he had come across the Lambrecht "Polymeter," and would like to know something of its relations to the Mason hygrometer, in respect of scientific accuracy.

Mr. Dixey said that the Lambrecht hair hygrometer, or polymeter, as the designer called it, could not compare as regards accuracy with Mason's instrument; but he (the speaker) thought the hair hygrometer very much more likely than the other to become popular. He considered that the hygrometric state of the air had really much more popular interest than the pressure of the air. Let them consider, for example, how a careful mother said, when the temperature was low and the air saturated with moisture. "It is too damp for the children to go out." As the temperature rose and the relative humidity became lower, it was commonly understood that the tender skin and the lungs of

children might be more safely exposed. From the Optician's point of view, there should be an even better sale for hygrometers than for barometers. A popular scale could be easily devised for the former instrument, showing the moisture of the air, not necessarily by so technical a measure as that of percentage, but by a blue and a red division of the scale indicating healthy and unhealthy conditions respectively, which the merest housemaid could be taught to understand. Someone might say, while an over-heated room with low relative humidity is bad (for instance), of what use is this information? How should the observed condition be corrected? This was a large subject and one which time would not permit the speaker to discuss. He might say, however, that the relative humidity could be raised by lowering the temperature, as by opening the window. Or a spray of water dropping on to ferns or other foliage plants could be used. To lower the relative humidity was not so easy. Perhaps the best plan, where it could be carried out, was to make use of large blocks of ice on which the superfluous moisture should be permitted to condense and then run away—not to be allowed to remain exposed to the air of the room in a stagnant condition so as to undergo evaporation again and, in process of time, make the air as wet as before. With respect to what Mr. Thornthwaite had said, the deposition of moisture was a constant source of trouble to Opticians. All of them had to deal with this trouble in connection with spectacle lenses, trial lenses, opera glasses, and so on. If a customer took up an expensive pair of opera glasses, and found that he could see through them nothing much more than a typical "London fog," he was apt to think that the instrument was after all a bad one. In each of these cases, the remedy was simply to warm the offending glass. Again he thanked those present for the reception accorded to his paper.

PRESIDENTIAL ADDRESS BY DR. C. V. DRYSDALE.

Delivered before the Optical Society on April 16th, 1903.

WHEN your late esteemed President, Mr. Hyatt-Woolf, kindly conveyed to me the Council's invitation to fill the position for the ensuing session I felt considerable reluctance to do so, and finally accepted with no little hesitation. Not because I was insensible to the great honour done me by the Council, or from lack of interest in the Society, but because I felt that the post ought to be filled by one of those gentlemen who, from their long association with the optical industry, or who from their activity in forming and promoting the interests of this Society, would be qualified to speak in your name. Of those who have already filled the position, Mr. Thornthwaite and Mr. Dixey have become identified with the optical movement from their long connection with it, and for the zeal with which they have supported everything tending to its advancement; and Dr. Walmsley, from his great experience of organisation and scientific knowledge, has been able to greatly further the interests of the Society. Mr. Hyatt-Woolf, again, from his continued intercourse with all branches of the optical trade, and his unfailing tact, has been able to preside over your meetings in a way which we must all admire. In none of these ways can I claim to be able to represent the Society. My part hitherto has been that of a student interested in scientific matters generally, and in optics somewhat specially, and also interested in imparting such knowledge as I have been able to glean to others who are seeking it. I consequently expressed this feeling to Mr. Hyatt-Woolf when he so kindly supported the invitation of the Council, but met with the reply that as the Council were fully aware of those circumstances when making their selection I ought not to allow them to stand in the way. If, therefore, in this Address, or during my tenure of office, I shall be found ignorant of some of the duties of the President of the Society or of the burning questions of optical politics which arise from time to time, I will ask you to kindly lay the blame in the proper quarter.

I cannot enter upon the duties of office without saying a word as to the other office to which we owe so much, that of the Secretary, an office on which so much depends, and to which frequently so little credit is given. Presidents may come and Presidents may go, but the work of the Secretary goes on for ever. It is the duty of the Secretary to take in hand each successive President, and to instruct him in the various duties of his office, to do all the work for which the President takes the credit, and to take the blame if anything goes wrong. I think it would perhaps be a good rule if the offices of President and Secretary were combined, as we should, at any rate, then have a fairer distribution of the credit; the only difficulty then would be that it would be indeed hard to find candidates for the position. Be that as it may, with Mr. Salt at the helm of this Society, one has the comfortable reflection that it is almost impossible for anything to go very far wrong. It is unnecessary for me to say more concerning Mr. Salt, as you have recently testified to the esteem in which he is held, and I can only say, personally, that my thanks are due to him at the commencement of my Presidential career, as I know they will be at its close.

I have started the Address with a personal explanation for a particular reason, viz., that, as I cannot pretend to speak this evening in your name, I hope that you will bear with me if I speak in my own.

It is often said that no one feels so confident on any particular subject as the one who knows nothing about it, and, although I have hitherto had little connection with the optical industry, I must plead guilty to having formed somewhat strong opinions on the present and future status of the industry and of this Society. I will ask you, therefore, and others who may possibly read this Address, to kindly remember that though I am speaking *ex cathedra*, in the literal sense, my words are not to be taken as representing the opinions either of the Council or of the Society as a whole.

In choosing the subject for such an Address as this, one is confronted at the outset with many different possibilities. The Address may take the form of a retrospect, a survey of present conditions, or a prophecy of the future; of congratulations or lamentations at our present position and future prospects; or, again, it may be devoted to some special subject which has come

under the writer's own experience. Valuable as the latter often is, I propose to deal with general rather than particular subjects this evening, partly because I have already had other opportunities of bringing special matters before the Society, but chiefly because in the present state of the optical trade and of our Society I think we are hardly ripe for Addresses of a very special character. I propose, therefore, in this Address, to deal as briefly as I can with the Past, Present, and Future of Optical Science and Industry, and to divide it into four parts, entitled respectively, The Scope of Optical Science and Art; Optical Progress in the Past; The Present Position of the Optical Industry; and, lastly, The Future of the Optical Industry.

The Scope of Optical Science and Art.

To many of us this title will sound a curious one, but I have chosen it for what seem to me the most cogent reasons. Most of us here call ourselves Opticians, and rightly so, in that some branch of optics is what we are interested in, and by means of which we derive our incomes. But, Ladies and Gentlemen, we are continually hearing of the competition in various branches of the optical trade, and anxieties are often felt lest some detail of optical work should escape into other hands. I therefore thought that it would be not unprofitable to pause for a moment and consider the legitimate sphere that is open to the Optician, and to enquire whether we make use of all, or even a comparatively large part, of the magnificent field which is open to us. I must ask your pardon for inflicting upon you what may seem to some an unnecessary scientific discourse, but I hope to show you the advantage of doing so, and the fault will lie with me if I do not present it in such a way that you will find it of interest.

It has been well said that the term Optician should connote one versed in the science of optics, and, if we enquire into the scope of optical science, we shall then know what field is open to the Optician. Now optics is, in strictness, the science of vision, and for vision four things are necessary: firstly, we must have a source of light; secondly, a medium through which that light is transmitted; thirdly, we must have bodies which reflect or refract that light; and last, but not least, we must have organs of vision to perceive it. As our organs of vision are sometimes imperfect, and as they do not possess the whole of the range or power we

desire, optical instruments are required which correct or extend the range of vision; and, as our unaided senses are incapable of exact measurement, instruments are frequently required which will enable us to make them. Moreover, in order that light may be produced and utilised with economy it is necessary that, like all other commercial articles, it should be capable of measurement. Finally, as these instruments cannot be produced without a certain amount of manual and machine labour, engineering is an indispensable auxiliary to optics.

If we now set down from these considerations the scope available to the Optician, it would apear to be as follows:—

(1) The production of light.
(2) Its measurement.
(3) Its transmission.
(4) Correction of visual defects (other than of a morbid nature).
(5) Extension of powers of normal vision.
(6) Recording of luminous impressions (photography).
(7) Optical measurement of various quantities.

On the other hand, if we turn to actualities, we find the sections (1) and (2) hardly appear to come under the province of the Optician at all. The great advances which have been and are being made in the production of light come from men whom we may term gas engineers or electrical engineers, and who certainly do not call themselves Opticians. Again, if we turn to the other parts concerning optical instruments, we find that although the production of these instruments has progressed to an enormous extent it has only done so to a comparatively small extent in this country. Mr. Conrad Beck has recently stated that:—

"The value of scientific instruments imported into this country in

"1897	was	about	£99,000.
"1898	„	„	£206,000.
"1899	„	„	£237,000.
"1900	„	„	£648,000.
"1901	„	„	£710,000.

"The value of the exports of scientific instruments in 1901 was £258,000, and has been at about this figure for five years."

There is no necessity for me to comment upon these figures, but I do not quote them here in order to afford an excuse for lamenta-

tions as to the decadence of the optical industry in Great Britain; I would rather use them with another motive, to point out that the scope of the Optician is so large that there is so much ground to be regained and pursued, that instead of wasting our time over petty trade details and recriminations, we should awake to the great dignity of belonging to the optical profession, and unite in adding to its laurels, since it is so easily possible.

Let us turn again for a moment to even higher considerations, and ask what place optics takes among the natural sciences. We all know that optics is stated to be a branch of physics, of the science which deals with the motions of bodies both in sensible masses and in ultimate particles, be they molecules, atoms, or electrons. The term physics will, in the course of time, probably be identified with the great science of electro-magnetism. Heat, light, and all electrical phenomena have been shown to be capable of being grouped together under this head, and probably before long the one unknown force of gravitation will have been brought under the same category. When we consider the science of electro-magnetism in general we find that it can be split up in four definite sections.

(1) Electricity at rest—Electrostatics.
(2) Electricity in linear motion—Electric currents.
(3) Electricity in rotation—Magnetism.
(4) Electricity in vibration—Alternate currents, Hertz waves, Heat, Light, Actinism, Kathode rays, X rays, and radiant matter rays.

Optics, therefore, forms a part of the last section, but it is so inseparably connected with the other parts of the same section that it may be said to embrace them, and we therefore have added to the legitimate extension of optical work, that of wireless telegraphy, and the ultra-violet radiations, such as X rays, Kathode rays, etc. It is obvious, however, that for the complete understanding of optical phenomena some knowledge of the other branches of electro-magnetism is required, and therefore we have a very strong association between the electrical engineering and optical industries, which has to a certain extent been recognised, more perhaps by the former than the latter.

Optical Progress in the Past.

The history of the development of optical knowledge is a most interesting one, and a surprising one in many details, not the

least surprising part being the extraordinary misconceptions which have been held in the past, and which have only in comparatively recent times been rectified. Among the Greeks, who carried geometrical science to such a pitch, the notions as to the nature of light and of vision were most obscure; so far as any coherence can be found in their writings they appear to have held the view that objects are rendered visible by emanations from the eye of the observer; and the obvious disproof of this, viz., that, if it were the case, objects should be visible in the dark, seems never to have even occurred to them. They were, however, acquainted with the fact that light travels in straight lines, and with the laws of reflection. From that time till the 16th century little progress seems to have been made in the subject, and it is only therefore in the last four centuries that the whole fabric of optical science and art has been built up. It is surely of interest for us to look into the records and to see what part in particular Great Britain has taken in the development of this important and beautiful science, especially when we find that this part has in the past been a leading, if not the leading, one, so that it may show us what yet has to be done and what a prestige we have to regain and to extend.

The growth of optics may best be divided into two main heads: first, that of the science of light, involving its nature and behaviour; secondly, in the practical applications of the science to the various instruments which extend our powers of vision. The optical industry is one which, above all others, owes equally to pure science and to practical work, and it is fortunate also that perhaps more than in any other industry this fact is recognised.

Turning first to the development of pure optical science, we find that the first quantitative law concerning the refraction of light, the constancy of the ratio of the sines of incidence and refraction became known to Snell of Leyden some time before 1626, but it was not until 1637 that it was first published by Descartes. Before that time, indeed, Alhazen had made measurements on the deviation produced by refraction, but had not obtained any regular law. The commencement of real research into optical matters started with our great philosopher Newton, whose experiments and deductions are among the most remarkable of even his remarkable achievements. Newton was the first to make use of the law of Snell to measure the refractive indices of various

bodies, to produce the spectrum, and to explain the nature of the different colours and their unequal refrangibility; he was also the first to discover and to make measurements on some of the effects of interference diffraction, the familiar Newton's rings being formed by him with the object of measuring the thicknesses of the air films required for different colours. These observations of Newton and of other philosophers, such as Young and Huygens, led to fresh speculations as to the nature of light, which resolved itself into the famous controversy between the theory of emission and the undulatory or wave theory. The former of these theories was held by Newton, although in order to explain the effects of interference and diffraction he was obliged to modify it in such a manner as to make it bear some resemblance to the modern wave theory, and as at that time the emission theory was able to account for a number of the phenomena of light which the wave theory could not, it had for a long time after the mastery. To Huygens must be ascribed the credit of the authorship of the wave theory as well as the discovery of the polarisation of light, but he was unable to apply his theory to account for most of the phenomena of light, nor did he suspect the important modification which has since been made to the theory, that the light waves are transverse in their motion, not longitudinal, as with sound waves. Meanwhile the discovery of the velocity of light by Romer in 1676, and of aberration by Bradly in 1728, had shewn obstacles to the emission theory. The principle of interference discovered by Young in 1801 gave fresh impetus to the wave theory, but the immense authority of Newton prevented his discoveries having due weight, and the definite proof of the undulatory theory must be ascribed to Fresnel, who, in 1815 and afterwards, reduced the theory of interference to mathematical shape, and also accounted for the effects of double refraction and polarisation. Since that time practically every one of the experimental effects of light have been explained, and, indeed, in many cases, predicted by the help of the undulatory theory, considerably through the instrumentality of Airy, Lord Rayleigh, and Lord Kelvin; but the culminating point was reached by Maxwell in 1856, when the connection between electro-magnetism and light, which had been suspected and experimentally demonstrated by Faraday, was developed by the former into the electro-magnetic theory of light—one of the most magnificent generalisations which

have ever been made. This theory, namely, that light simply consisted of electro-magnetic oscillations of particular rate, was at first doubted, and it is to the late Heinrich Hertz that we owe the final experimental demonstration of the theory—whose work, by the way, has been so fruitful in the other direction of wireless telegraphy.

In all this work England has taken a leading part in the development of new theories and in their verification, although in many cases the latter work has been done in the greatest detail on the Continent.

Let us turn now to the subject which perhaps may seem to more deeply concern us, that of the technical application of optical science, and we cannot do better than consider first of all the evolution of the refracting lens. When and where lenses were first used it is impossible to say; it it certain that the ancient Egyptians not only had the art of making glass, but that they employed glass globes filled with liquid for magnifying purposes, and, that wherever glass-making has been carried on, lenses consisting of beads of glass have been employed. With the application of lenses to various optical instruments we shall deal shortly, but we will now follow the improvements in the lenses themselves. It does not appear to be known who first produced lenses by grinding and polishing, but about the middle of the 16th century spectacle lenses were produced in this way. The discovery of the telescope about the year 1608, however, called for lenses capable of producing clearly defined images; but nothing was done, except in the improvement of the accuracy of grinding, until the time of Newton, whose discovery of dispersion led him to the recognition of the important error of chromatic aberration in lenses. Newton also was sufficiently aware of the conditions necessary to produce an achromatic lens, but as his somewhat rough measurements led him to conclude that all materials dispersed proportionally to their refraction, he concluded that the making of an achromatic combination was impossible, and he therefore devoted his attention to the production of the reflecting telescope. It was reserved to Mr. Chester Moore Hall, in 1733, to produce chromatically corrected lenses, but as no publication was made of his discovery it was unknown at the time. In 1747, Euler, the great German mathematician, laid down the mathematical conditions for correcting both chromatic aberration and

the error known as central spherical aberration. Although his calculations, when put into practice, appeared erroneous, this, however, appears to be due to want of knowledge as to the real nature of the glass employed; and Dollond, in 1758, making use of Euler's principles, was able to produce objectives corrected both for chromatic and spherical aberration. From that time onward the theory of the correction of the chromatic and spherical errors of different lenses was actively carried on in this country by Herschel, Airy, and by Coddington, who, in 1839, published his well-known work, the standard English treatise on the subject. Up to this time the development of optical instruments rendered possible by the greater perfection of lens construction had been greatly due to investigators in this country, but from that time since it is regrettable that the attention of British scientific men has been so devoted to the extension of the purely scientific portion of the subject that the problem of scientifically improving the forms of lenses has been almost entirely taken over by the Germans, who were before that time very considerably behind us. Since then, the progress of the optical industry in Germany has been most rapid, owing to the mathematical labours of many eminent physicists, notably Petzval, who in 1843 gave the principle for securing a flat field; Steinheil; Abbe, to whom we owe the important sine condition and the commencement of the diffraction theory of image formation in the microscope; Rudolph, who first produced a lens having a flat field free from astigmatism; Lummer, who gave the general principle for correcting distortion; and Von Seidel, whose mathematical labours resulted in the development of an almost complete theory of the errors or aberrations due to lenses, which has proved of inestimable value in design. The researches of these mathematicians, however, would have been fruitless had not Abbe and Schott rendered their application possible by the production of glasses especially produced to fulfil the mathematical conditions required, the immense labour and scientific research required for which may be gauged to a certain extent from the recent book on Jena Glass by Dr. Hovstadt, whose work has just been translated into English.

Of the specific technical applications of optics three stand out most prominently, those to the production of telescopes and microscopes, and to photography. The origin of the telescope is to some extent shrouded in mystery, as it has been held with some

show of reason that the principle of its construction must have been known to Bacon, and, later, to Digges. The first definite instrument, however, appears to have been produced in Holland in 1608, although there appear to be three claimants for the honour. Gallileo, in 1609, having heard that such an instrument had been produced, re-invented it, and his form of telescope has since been generally known as the Gallilean glass. In 1666, Newton, as before stated, produced a reflecting telescope with which he obtained good results, and Cassegrain followed, in 1723, with another form of reflector. In 1723, Hadley produced a 6-in. reflecting telescope on the Newtonian principle, while in 1758, Dollond produced achromatic refracting glasses. The greatest advances, however, in astronomy were first made with reflecting glasses by Herschel, who, in 1774, produced his 7-ft. glass, in 1783 the 20-ft., and in 1789 the great 40-ft. reflectors. The refracting telescope had been developed but little in the meantime, owing to the difficulty in obtaining suitable glass in large, clear discs, and it was not till 1805 that Guinand, in Switzerland, improved the methods of producing optical glass, carrying them on afterwards in conjunction with Frauenhofer, and finally later with his nephew; while in England the production of optical glass was first started by Messrs. Chance in 1848, using Guinand's methods, which were introduced by M. Bontemps. Since then the making of larger and larger refracting telescopes has gone on without intermission, reflectors being entirely superseded, culminating in the great Lick telescopes and the colossal instrument shown at the Paris Exhibition.

In another direction, however, the development of the telescope has some interest for the Optician. In 1853 Porro introduced a glass in which the erection of the image and shortening of the body of a telescope was obtained by the use of reflecting prisms. Messrs. Zeiss, in 1893, first made use of this principle in the production of their prism glasses, which have since been so widely used, and which bid fair to supersede every other form of field-glass.

Turning now to photography, we have already to some extent referred to the optical side of the subject in our reference to the general subject of lenses, and we are all aware that great advances in this direction have, till recently, come to us from Germany, with the exception of the lenses produced by Messrs.

Ross, under the direction of Dr. Schroder, who was indeed the first to make use of the anomalous Jena glass in his concentric lens. One great improvement of modern times must not be forgotten, that of the production of the telephoto lens, which was independently invented by Mr. Dallmeyer (in England), M. Dubosco (in Paris), and Prof. Miethe (in Germany), in 1851.

A brief reference only must be given to the other side of photography, viz., the production of the image, as it is hardly strictly optical in its character. Scheele somewhere about 1780 was the first to notice the darkening of silver chloride on exposure to light, and in 1801 this was further extended by Ritter, who noticed that it was to the violet and ultra violet parts of the spectrum that this change was probably due. In 1802 Wedgewood produced prints by means of silver nitrate, but was unable to fix them. From 1814 to 1827 Niepce produced pictures by means of asphaltum, and after working in conjunction with Daguerre for some time the publication of the process known as the Daguerreotype took place. On the 25th January of the same year Fox Talbot has shown that papers subjected to alternate saturation with silver nitrate and salt gave good images, and in 1840 Herschel introduced fixing by hypo-sulphite of soda. In 1841, Fox Talbot also invented the process which he termed colour type, in which gallic acid was employed in development, and shortly afterwards the nephew of Niepce, who had been working with him, introduced the important improvement of iodised albumen. In 1850, Le Gray, Archer, and Fry simultaneously introduced collodium, and, in 1864, Woodbury introduced the carbon process, although Swan independently did the same thing. From that time onwards a large number of processes have been developed, the important improvement of the dry plate being introduced by Gaudin, and by Muirhead, in 1854. Two other branches of photography have, however, considerable optical interest: one is the photo mechanical, or half-tone, process, which has within the last ten years effected such a revolution in illustration, and with which Mr. Ives has been so greatly associated; the other, that of colour photography. With reference to the latter, Seebeck in 1810 noticed that the colours produced by exposing silver chloride to the spectrum bore some resemblance to those to which it had been exposed, but this lay dormant until 1839, when Herschel independently discovered a similar effect. In 1848, Becquerel,

using the Daguerreotype process, found that colours could be obtained, but up to the present no commercial method of producing photographs in natural colours by direct process has been found. On the other hand, the theories of colour vision built up by Young, Maxwell, and Helmholtz, have shown that every colour can be matched by a suitable combination of three primary colours—red, green, and violet—and Maxwell himself produced images in colour by photography through three-colour screens. This tri-chromatic process has been further extended and improved by Ives in America, Joly in Ireland, Lumiere in France, and Abney and Sanger Shepherd in this country; and it has also been applied to the important industry of three-colour process work. On the other hand, successful attempts at direct colour reproduction have been made by Lippman in Paris, and by Prof. R. W. Wood, whose diffraction process was shown over here a few years ago.

Coming to the microscope, we find that in this direction English Opticians have played a very important part. The first compound microscope, like the telescope, appears to have emanated from Holland in 1590, being produced by Messrs. Hans Zansz, Spectacle Makers, the errors, however, inherent in the single lenses made the compound microscope of but little avail, and for some years afterwards the simple microscope was employed. The first attempt at improvement was made in 1812, by Amici, who attempted to apply Dollond's achromatic lenses to form an object glass, but, failing, produced a reflecting microscope. In 1820, Messrs. Selligues and Chevalier, in Paris, took up the suggestions of Amici, and produced the first successful achromatic objective, and in 1825 Mr. Tulley independently produced an objective in which the chromatic aberration was exactly corrected. In 1830, Mr. Lister introduced the valuable principle of employing the aplanatic points of the spherical lenses, which was afterwards adopted by Amici, with the further improvement of the immersion system. The late Andrew Ross was the first to adopt Lister's suggestions and to carry them out to such an extent as to discover the effect which the cover glass thickness had on the quality of the image, and to invent the correcting adjustment, and his work marks the beginning of the more perfect optical work in microscope construction. Meanwhile, Amici's suggestion as to immersion had

been taken up by Messrs. Hartneck and Nachet, using water as the fluid; but to Wenham we owe the first suggestion to use homogeneous immersion, adopted later by Zeiss. Since then the greatest improvements have been made by Abbe, who successively discovered and applied the sine law, homogeneous immersion, the limit of resolving power due to diffraction, and the principle of apochromatism. On the other hand, in this country, many fine objectives have been made by Messrs. Powell and Lealand, working not so much by theory as by extremely skilful experiment, and other British manufacturers, notably Messrs. Swift, have produced good objectives.

Finally, we come to that portion of optics in which most Opticians are specially interested, viz., that of Ophthalmology. In this direction the leading position has been taken in the past by Continental scientific men, notably Helmholtz, Donders, Sanson, Purkinge, Snellen, and Tscherning, although much good work was done by Young and by Airy in this country. Within the last decade, however, great interest has been shewn in this subject, both in Britain and America, and the honour of leading now probably lies between them. The numbers of different tests for visual defects which have sprung up recently, and the number of instruments which have been devised for the purpose, show to what extent interest has been aroused, in fact, to this subject, perhaps more than any other, we owe the revival of interest among Opticians, and their demand for further education.

The Present Position of the Optical Industry.

Let us now pause for a little and see to what stage each section of the optical industry has reached. In order that we may appreciate this to the fullest extent, I have asked the assistance of the principal optical firms in this country, who have one and all kindly responded to my request, that they should show us their highest achievements in any special direction they have taken up. An inspection of these things is of far greater importance than any words of mine, but perhaps you will allow me to briefly sum up the position.

In the production of light itself the three greatest achievements of recent years have been the invention of the Welsbach mantle, the Nernst Electric Lamp, which has applied the prin-

ciple of high incandescence used in the Welsbach mantle to electric lighting, and last, but not least, the Cooper-Hewitt Mercury Vapour Lamp, which, by the special kindness of the British Westinghouse Company, I am enabled to show you this evening. The two latter lamps are of such extreme interest that a word or two of explanation may be devoted to them for the benefit of those who may not have hitherto studied the subject. It has been well understood by physicists for some years past that the only method of obtaining an efficient light is to be able to produce molecular agitations of the luminous body of the proper frequency for vision, viz., between 400 and 800 billions per second. To do this there are two principal ways open to us, either we may produce the light through the agency of heat, as in all our ordinary illuminants, or we may endeavour to produce these molecular agitations directly by electric discharge in gases. For the former of these methods to be efficient, however, one thing is extremely important. It is well known that if a body is gradually heated it passes through various stages of temperature until it reaches a dull red, after which the light becomes successively orange, yellow, and so on to white or a slightly violet colour. We know that this corresponds to an increase in frequency of vibration of the particles, but it must be remembered in this case that the vibrations are by no means pure. A light of a single colour, which we term monochromatic light, has some analogy to pure tone, such as is produced by striking a single note on a piano, but in order to produce a vibration of sufficiently high frequency to produce vision by the agency of heat it is necessary for us to have at the same time all the lower rates of vibration, in fact, it is as if to produce the sound of a high note on a piano it was necessary not only for us to depress that key but also every key below it. A perfectly efficient lamp would be one in which all the energy given to it was converted into vibrations between the limits of vision, and it should preferably have all the intermediate rates of vibration. It is obvious, therefore, that to produce an efficient light by the agency of heat all we can do is to increase as far as possible the ratio of the luminous portion of the vibration to the total, and this can only be done by increasing the temperature so as to get as many vibrations as possible in the upper octaves. The knowledge of this led Dr. Auer Von. Welsbach to employ a

mantle of highly refractory material, heated to the highest possible temperature in gas lighting, with the economy we know so well, and Dr. Nernst similarly wished to apply the same principle to electric lighting. Up to that time the only material that had been used was carbon, owing to the fact that carbon had the highest fusing or volatilising point of all conducting materials. The rare earths employed by Welsbach in his mantles were not conducting, and had not therefore been previously used for electric lamps, but Dr. Nernst found that they were conducting if raised to a sufficiently high temperature. The outcome to this has been that the Nernst lamp consists of a rod or filament of material of the same nature as the Welsbach mantle, and is heated when first switched on by a separate coil, which is switched out as soon as the filament is heated enough to be conducting. A number of other ingenious details are included in this lamp, but cannot be described here. On the other hand, the Cooper-Hewitt lamp is an example of the second method, that of obtaining light by discharge through gases. The lamp is essentially the simplest means of producing light ever known, consisting, as it does, simply of a glass tube, usually of from 2 ft. to 3 ft. in length, and about one inch in diameter, with a bulb at the bottom in which mercury is placed. Two connections are made to the top and bottom of the tube, and an auxiliary connection at the side. The tube is then exhausted and is constantly filled with mercury vapour. By an ingenious device which cannot here be described, a high pressure discharge is started through the tube at the moment of switching on, after which an ordinary pressure of 100 to 200 volts is sufficient to keep up a perfectly uniform light of very high efficiency. The efficiency of this light is stated by the Westinghouse Company as about 2 candles per watt, and corresponds, according to Herr Geer, to a nett luminous efficiency of 48 per cent., showing that, although we have not yet reached the efficiency of the light produced by the glow worm, we are at any rate getting nearer. The only objection, and perhaps a fatal objection from a domestic point of view, is the fact that the light is restricted to three colours in the orange, green and violet spectrum, and consequently utterly changes the colour of most objects viewed by it. The absence of red rays, of course, makes all red objects look black, crimson appears blue, etc., as you will see for yourselves

this evening. Notwithstanding this, the absence of red rays makes the light extremely restful to the eyes, and its steadiness and economy should undoubtedly make it welcome wherever the correct rendering of colour is not of importance.

The efficiencies of various sources of light have been investigated mainly by Langley, and Herr Geer has recently given us a table of the efficiencies of modern illuminants, as follows :—

Argand Gas	1.6	per cent.
Glow Lamp	6	„
Arc Lamp	10	„
Acetylene	10	„
Geissler	32	„
Mercury Arc ...	41 to 48	„
Glow Worm	95	„ Langley.

Putting aside, therefore, for the moment, the Geissler and Mercury lamps, we see that we have no source of light which gives more than one-tenth of its energy in useful form. This is sufficient to show what great improvements ought to be made in the future. The mercury lamp is a great step in advance; while, on the other hand, the bull has been taken by the horns in the production of a bacterial lamp, in which a light of probably a high efficiency is produced by inoculating gelatine with light producing bacteria. This light is stated to be eminently suited for mines.

Of the other departments of optics we find that in the matter of telescopes practice is almost settled. Increase in the size of our large telescopes will doubtless go on with the power of producing larger uniform discs of glass, and, on the other hand, great improvements have recently been, and will continue to be, made in the reduction of the secondary chromatic aberration. Probably the chief difficulty in this direction will now be found to be mechanical, in the supporting of the telescope without vibration; the use of a coelostat in the large Paris telescope is an indication of this. In the smaller work the greatest improvements at the moment are undoubtedly in the matter of prism glasses, of which new forms are frequently being brought out. The improvements are, however, mainly in mechanical directions, and not in optical ones, although great improvement is to be desired in the matter of illumination. With reference to the latter, however, I am able by the kindness of Messrs. Aitchison to

show you the new form of prism glass which they are about to put on the market, which is probably the most important step since the first introduction of prism glasses. The inferior illumination of ordinary prism glasses, due to small aperture, has long been known, and Messrs. Aitchison in starting to manufacture glasses in this country commenced with the object of improving this defect. The result has been to produce a glass in which the large field and other advantages of the prism form have been secured with object glasses approaching the size employed in non-prism glasses, while, by the use of a variable diaphragm, the aperture and definition can be controlled as desired. The advantage of this glass over ordinary prism glasses on badly illuminated objects is very marked.

In photographic lenses progress is still being made in the direction of producing anastigmatically corrected lenses of much simpler design than heretofore, four single lenses being in many cases employed. At the same time, the advent of colour photography and three-colour work has rendered perfect colour correction of the objective of the highest possible importance, and the modern lenses are therefore frequently apochromatic.

Of Continental makers, the firm of Zeiss have long been known for their Protar, Planar, and Unar Lenses, and within the last few months they have added their Tessar Lens in which remarkably good definition and freedom of distortion is secured with simplicity of construction and, when necessary, apochromatism. Of other Continental makers, Messrs. Goerz, Voigtlander, and Busch produced lenses of somewhat similar character. The last few years have seen a great development in photographic optics in England. The firm of Ross, besides making a number of Continental forms of lenses, have made several good forms of their own, and have just recently put on the market their new homocentric lens, for which they claim perfect anastigmatic correction with semi-apochromatism. Messrs. Dallmeyer and Aldis were the first in England to produce convertible lenses in their stigmatic lens, besides their achievements in tele-photographic lenses. Mr. Dennis Taylor, one of our greatest mathematical Opticians, may be considered to be the pioneer of the modern simple forms of anastigmats with his Cooke-Taylor lens, and his latest achievement is in the focusing lens, whereby considerable range of focus can be obtained within the lens itself. Messrs. Watson have pro-

duced an excellent lens in their holostigmat convertible lens; and last, but not least, Messrs. Beck, working in conjunction with Dr. Steinheil, have produced the Beck-Steinheil orthostigmat of such renown, besides several other lenses. In America one lens may be mentioned—the Bausch and Lomb plastigmat.

Turning to colour photography, we are able to see this evening what great advances have been made in all directions. While we cannot yet claim to have obtained a direct colour reproducing process which is commercially workable, we may yet congratulate ourselves on the results obtained by the tri-chromatic process, in which the faithfulness of colour reproduction is now almost perfect. The work of Messrs. Lumière and Sanger Shepherd, which you see this evening, is a striking example of what is being done, and the new paper printing process of the latter is of especial interest. I am also able to show you, through the kindness of Messrs. Penrose, a few examples of Lippmann photography, and Mr. E. Senior has kindly lent me some excellent examples of his work in this direction, and a few illustrations of the Joly three-line process, which has, however, I regret to hear, been recently withdrawn.

In projection work little need be said except that progress is steady. The most interesting recent development is perhaps that of Messrs. Zeiss, with their Epidiascope; and Messrs. Newton have also recently brought out a new projection microscope of great perfection.

Finally, dealing with microscopy, we may say that things have reached a stage where development is only possible in detail. The researches and productions of Prof. Abbé have brought this instrument mechanically up to almost the limit of optical perfection, especially with the employment of the mono-brom-naphthalin immersion, and further improvement seems to lie rather with the illuminating than with the observing apparatus.

Great interest is being taken at the present time in the diffraction theory of microscopic vision on which any future improvements depend, and, in conjunction with this, the extremely interesting announcement which has just come from Germany, that Messrs. Siedentopf and Zsigmondy have succeeded in obtaining a magnification of 50,000 diameters, and thereby to actually see the diffraction images produced by groups of molecules, is of especial significance. It is, of course, impossible to see the mole-

cules themselves in the strict sense, as with transmitted light the waves can re-unite after passing them, as with a small object in the sea. By reflected light, however, portions of the wave may be turned aside and therefore make the presence of the molecules manifest. Further details of this matter will be welcome when available.

I have not alluded in detail to the development of the ophthalmic side of optics, as to do so would require a catalogue of names and inventions in itself. Suffice it to say that in this direction progress has been rapid and satisfactory.

The Future of the Optical Industry.

Having now seen in an approximate manner where we stand at the present day, let us look forward a little and see what the future of the industry is to be, based on our experience of the past; and with the future of the industry I would couple the future of the Optical Society. In the larger engineering industries, their advancement has been marked by and inseparably connected with the progress of the Societies or Institutions which they have formed, not only for the protection of their interests but for the discussion of the scientific and others matters which pertain to their industries; and I cannot help thinking that if there is to be any real progress in British technical optics it will be through the medium of some such Society, and there can be no question that this Society is the one which should hold that place. We have seen that Great Britain has taken what may be termed a leading part in the development both of the scientific and technical optics in the past, and the fact that she has of recent years lost that lead in the domain of technical optics has been due undoubtedly to the separation between scientific and manufacturing work, British scientists having devoted their attention practically entirely to the development of the higher physical theory, leaving the question of the improvement in lenses and optical instruments almost alone. I suppose it will be unquestioned that to regain our position and to take the lead once more—as I firmly believe can be done—the work lies with the manufacturer, coupled with the scientific aid necessary to produce the best results; and, again, it is to the distributors we must look to tell us in what direction progress is most wanted, what instruments are being required, and in what form they

are being sought. For progress therefore to be speedy and satisfactory it must be by the union of the three great branches: science, production, and distribution; and these three must meet under the auspices of some such Institution as this to interchange their views, for the mutual benefit. For any advance at all it is absolutely necessary that productive work should be backed up by the highest possible scientific skill, and it is pleasant to note that this is at length being realised in this country; but, though such reliance and dependence should be placed on scientific methods, it will not do for manufacturers to blindly follow the precepts of scientific men without an intelligent understanding and criticism of the results themselves. For many years the scientific branch of electrical engineering was behind the practical development of it, and in some directions this may even be said of optics. The system of reckoning in curvatures and convergences instead of focal lengths was devised by Opticians for their benefit in spectacle work; and, as I have before stated, it is my deliberate opinion that this method is superior to the one hitherto adopted by scientific men in almost every particular, and this method can be applied with advantage to the higher work in technical optics as well as to the explanation of the simpler problems of reflection and refraction. For my part, I should be sorry to see the coming revival of optical knowledge in this country brought about by a pure copying of the methods which the Germans have employed, however successfully, in their work, although a study of them is essential and cannot but repay everyone interested in the subject.

From the scientific point of view the chief problems which present themselves for solution are those relating to improved methods of producing light and of measuring it when obtained, in extending the aperture and obtaining increased precision in optical instruments, and in the direction of securing better and more simple processes in colour photography. From the manufacturing point of view, too much weight cannot be given to improved methods of production; the making of both metal and glass components by automatic processes as far as possible, and, further, in systematising the production so as to do everything in the most efficient manner. Let us not forget, too, that in the engineering work of the present day great attention is being justly devoted to the conditions of labour. Our American

cousins, who were the first to realise the enormous advantages to be reaped from automatic production, have also had the acuteness to perceive that they are none the less dependent on the skill and intelligence of their work people, and they have perceived that to attain this in the highest degree it is necessary not only to give them facilities for developing their minds, but to secure the best conditions under which they live and work. This lesson is being but slowly learned in this country, where new ideas travel comparatively slowly; but, in the optical trade, where, in my opinion, the prospects are better than in almost any other industry, I hope that its revival will be attended not only with the realisation of the higher optical problems and improved manufacturing methods, but that our manufacturers will understand that for their own interests it is advisable to make the conditions for their operatives as good as is practicable; and, if possible, to secure identity of interests by profit sharing, or bonus-earning schemes. Not that it should be done in any way as a matter of charity, but simply that employers should carefully consider the question, and consider impartially how far it will serve their own interests to effect such changes.

Having given my reasons for attaching such importance to the work of the Society, let us see what it has done and what it must do to fulfil its mission. In the matter of the membership I am glad to say that progress has been steady, a slight nominal decrease in the membership roll being far more than counterbalanced by the fact that a considerable number of those who joined at the outset were only nominally members, whereas at the present time the membership roll fairly represents the position of the Society. In another direction, that of assisting in the standardisation of various branches of the trade, important work has been and is being done, and we shall all look forward with expectation and confidence to the report from the Standards Committee, which will appear later in the year. The Social Meetings of the Society have been well attended, and have brought together many different sections of the Trade. For all these things there is matter for congratulation and confidence; but I have tried to show you my reasons for believing that it is to the scientific side of this Society we must look for real influence upon progress. We have had many papers given in the past at these meetings which have been of undoubted value, but

I would submit to you that it is not merely by the listening to these papers or by the reading of them afterwards that great progress will be made. We must look to large scientific meetings and animated discussions on the subjects brought forward by various branches of the trade and by scientific men, in which the matters brought forward in the papers are not simply listened to or accepted, but are criticised and sifted that we may be certain of their truth. In order to encourage this, the Council has, I am glad to say, decided to ask you to sanction a change in the membership, by which three classes of members will be made instead of one, as at present. One of these is the creation of a limited class of honorary members, in which class we hope we may have the honour of enrolling some of our distinguished scientific men, while the other class will be termed the students' class, in which assistants and others under the age of 25 would be enrolled at a lower fee. In connection with this it is proposed to hold frequent informal meetings in the room of the Society, at which papers or other matters will be brought forward with a view to discussion by both the students and other members, and I sincerely hope that this will not only be done, but that it will be carried out with enthusiasm. To further inspire this scheme, I have suggested the publication by the Council of information as to the subjects on which papers would be welcomed, and I hope that it may be possible to publish this list, accompanied by an announcement of prizes which may be offered for the best papers. There must be many in our Society who have experiences of a nature to educate or interest others, and I hope that we shall see them coming forward with their views, both in the form of papers and contributions to discussion; and that we shall thereby find animated discussions take place both at the informal and the formal meetings. With reference to the latter, I have also suggested that the publication of advance proofs of the papers when possible, would also greatly conduce to discussion, in that those wishing to take part in it should come better prepared. If these things are carried out I see no reason why this Society should not rank among its numbers members of every branch of the trade and men of the highest scientific standing, and I sincerely hope that if this is the case we shall find everyone willing to co-operate in doing his best for the industry at large. In the optical, more than perhaps any other, trade I

venture to think that petty trade secrecy is ill advised. If those who spend their time over the hoarding up of some small, often imaginary, advantage over their fellow manufacturers, would spend that time in increasing their knowledge and diffusing it for the benefit of others, the gain to themselves would, I am confident, be greater, and the general gain to the industry would be enormous. In Germany this spirit of co-operation has been shown, even in extending their information to rival countries, and surely not without success; and if increased knowledge, coupled with the facilities of our patent laws, do not sufficiently protect the Optician, in my opinion he has chosen his profession unwisely.

This Address cannot well be closed without a reference to the question of education, in which the Society in particular, and the industry in general, is so honourably interested at present. I say honourably, because to the Optical Society, and to the Optical Trade as a whole, the credit must be given, so far as I am aware, of being the first industry that has so thoroughly realised the necessity for education as not only to ask for it but to back up everything that has been done with its greatest zeal. With such spirit it can hardly be doubted that anything which is done under the auspices of the trade will be a success, especially if, while taking this interest in it, they refrain from imposing more restrictions on the efforts of those who have the education to do than can be avoided. I had hoped that before this Address came to be delivered something would have been definitely known as to the proposed Optico-Technical Institute, but as this matter is still pending consideration it is impossible to go into any detail. While on the question of education, I would draw your attention to the remarks made by Mr. Angus in the very able paper he gave you some time ago on "The Secret of German Progress." As one who has had to undertake the education of a considerable number of evening students, I feel very strongly that something should be done to relieve the necessity for young people to obtain their education in the evening after a full day's work, and I hope that the day may come when we shall see some such system as the apprenticeship system in Germany in vogue, in which apprentices spend part of their time in the workshops and part at a technical institute. It says a great deal for the zeal of many of our younger generation

that they will voluntarily attend classes on three or more evenings a week, and do the necessary preparation work in addition to their often arduous daily duties, but we must not forget that this cannot but have a prejudicial effect in many cases on their physique. At the inception of the career of the Optico-Technical Institute perhaps we may see some step in this direction.

Ladies and Gentlemen, I fear that I have wearied you with this long discourse, and my only apology for it is that I cannot help feeling that this year will probably mark another great step in the history of the optical industry. During the last few years we have seen a great awakening of interest in technical optics, both in the trade and among scientific men, leading to the formation of the British Optical Association, the examination scheme of the Spectacle Makers Company, and the formation of this and kindred societies. All these have been making steady progress, and this year will see the report of our Standards Committee, and, I hope, the inauguration of changes in the arrangements of the Society which should lead to increasing membership and enthusiasm. The Optico-Technical Institute should shortly be commenced, and much is being talked about the formation of a national or international Optical Convention.

I may seem to be over-sanguine, but I see no reason why this Society should not increase its numbers until all who are connected with the optical trade are associated with it, and that the papers and discussions held at its meetings will so stimulate the trade that we shall see optical works arising commensurate with the largest on the Continent, carrying on work with the highest scientific and mechanical skill, and that we shall make our own glass on the same scale and with the same skill and perfection that it has been made on the Continent. I hope also that Opticians who have been educated in the nature and phenomena of light will devote their attention to its production, and that we may soon see great advances in this direction. Further, I would most strongly urge, in the interests of the industry, that great attention should be given to the testing of all optical instruments, so that their various errors should be capable of numerical expression. Without this, we can never be certain of producing the best results, and the survival of the fittest.

With these things before us it cannot but be well that we should

look back to the past, see how we stand at present, and look to the future with a confidence justified by our preparedness to direct it.

Mr. Hyatt-Woolf said that it had been an unfailing precedent with the Society, and one that he proposed to observe, not to subject the President's address to criticism. He had been glad of this himself last year when filling the position of President, when he had perhaps laid himself open to criticism which he would have been quite incapable of bearing. Still more was he glad of the tradition on the present occasion, when it fell to his lot to deal with an Address so exhaustive and so admirable, on which he would be quite unable to offer any criticism if that were called for. In passing from this question of criticism, there was just one point on which he might make an observation that might be of interest to those present, and might even not have occurred to the author of the address. He referred to the question of economy in connection with the Nernst electric lamp. The beauty of the light afforded by it was not to be denied, but he doubted whether in practice the economy would be so great as appeared to have been suggested by Dr. Drysdale. People with whom he was acquainted had found that the Nernst lamp was not in fact more economical than the ordinary form of glow-lamp, owing to the fact that the incandescent body of the Nernst lamp was exceedingly fragile and its cost relatively high—being as much as 2s. 6d. With reference to what had been said as to the apprenticeship system in Germany, he (the speaker) could give that his cordial support. He hoped that something would be done in the way of re-introducing an apprenticeship system in this country. At that period when our country had been paramount commercially, the system of apprenticeship was thoroughly established. He referred to the time when the Spectacle Makers Company was founded. Now it was the endeavour of the trade-unionists to limit apprenticeship; but, in the old times, heavy liabilities were thrown on masters in this matter; and that Guild with which the optical Trade was most nearly allied undertook the inspection of workshops, and saw to it that there were apprentices in sufficient number for the requirements of the industry. He sincerely hoped that the apprenticeship system might be revived in this country. And he believed that all must crave (as all of their members had sought to aid) the establishment of the Optico-Technical Institute to which Dr. Drysdale had referred. As their latest Vice-Presi-

dent, it fell to him (the speaker) to propose a formal vote of thanks for the Presidential Address to which they had just listened; and certainly no duty could be more pleasing to him. He remembered well the self-deprecatory way in which Dr. Drysdale had accepted the office which was urged on him; and he could not but remark how Dr. Drysdale had given the lie to such depreciation, by the splendid paper he had just accorded them. Dr. Drysdale had proved himself thereby to be a brilliant scientist, and at the same time an acute and far-seeing business man. And, remembering that their President was still one of the junior members of the Society, this achievement could only be looked upon as marvellous. He would conclude by proposing that they should give to Dr. Drysdale the heartiest vote of thanks ever accorded to a President of the Optical Society.

Mr. Dixey said that he had the greatest pleasure in seconding the vote of thanks so eloquently proposed by their late President. He would like, as representing the feelings of the Society, to assure Dr. Drysdale that this vote was not merely a formal matter, but was a true expression of the hearty gratification with which they had all listened to his paper. While the address ought not to be discussed (as their late President had just said), there were many subjects that had been alluded to on which they might have liked to elicit further information. However, it was the more easy to refrain from comments and questions at the present time as they might feel sure, judging by Dr. Drysdale's generosity in the past, that they would have other opportunities in future of, so to speak, "tapping his brains."

Mr. Hyatt-Woolf then put the motion to the meeting, and it was carried with the utmost enthusiasm.

The President said he could only thank most cordially those who had proposed and had so favourably accepted the vote of thanks. There was little else that he could add, at the moment, to what he had already said. He sympathised with Mr. Dixey's desire for further information; and he could well imagine that there were a good many criticisms which might have been offered, as many of the points that he had touched on were distinctly of a controversial nature. He hoped that if his paper proved to have any value, it might be through the inspiration of those present with a greater enthusiasm for their subject. The optical industry was not one of those in which it sufficed merely to go

on from day to day, making little improvements, but it was an industry in which there were large advantages to be gained, and great results to be achieved. With respect to Mr. Hyatt-Woolf's remarks on the Nernst lamp, it must be admitted that the filaments were certainly very fragile. If the lamp could be kept burning without intermission, as in the case of a laboratory experiment, it would be found to come out well in the matter of durability. The frequent switching on and off of the current, with the attendant expansion and contraction, was what hurt the Nernst Lamp. He might say that the last year or two had seen some revolutionary changes in methods of the production of light—which was his reason for alluding to the subject. Why should not this be done again? Why should not still further improvements be devised? And why should not this be done by some of those there present?

SOME MODERN DEVELOPMENTS IN COLOUR PHOTOGRAPHY.

Paper by Mr. A. J. Bull.

Read before the Optical Society on May 18th, 1903.

BEFORE considering some of the modern developments of colour photography I would like to show you some results of the earlier work.

To reproduce in a photographic print the colours of the original subject has always been the desire of Photographers since photography itself became a possibility, and at the present day the problem is at any rate partially solved.

Many methods have been tried to obtain the image in colour, notably by Carey Lea's coloured photo-salts, and by Lippmann's most interesting process of producing stationary light waves; but practically colour photography, as it stands at present, dates from Maxwell's application of the tri-colour theory of vision.

The essentials of this theory, which was enunciated by Thomas Young and worked out by Clerk Maxwell, are that there exists in the eye a triple colour sensitive apparatus, each element of which responds most freely to light of a particular wave length, and less and less freely as the wave length differs from that of the particular fundamental wave length. The three fundamental points are situated in the red, green, and blue of the spectrum, hence these colours are called the primary colour sensations, or, more frequently, the primary colours.

Curves representing the effect of each wave length upon the three primary sensations have been plotted by Maxwell, Koenig and Abney. Abney's are probably the nearest to the truth, since in these the three curves are approximately symmetrical about their maxima, and resemble the curve giving the relation between the response produced in any body having a definite free vibration rate and the frequency of the impulses producing that

response. The fact that the red sensation begins to be stimulated again beyond the blue, producing with the blue the sensation of violet, is probably due to the fact that the frequency of violet light is about one octave above the red frequency, so that response of the red sensitive elements re-occurs. And the rather curious phenomenon, although we do not usually look upon it as such, that out of the whole range of electro-magnetic radiation—Hertz waves (wireless telegraphy), heat waves, light waves, actinic waves—only about one octave is seen by the eye, is in all probability not because the retina is not sensitive to them, but because they are absorbed by the vitreous humour.

Maxwell saw that, since all colours could be matched by the suitable admixture of three, if one could take photographs giving records of the distribution of these three primary colours in a picture, then the colours of the original could be reproduced if the three photographs, illuminated by the same coloured lights, could be optically superposed so that the added effect of the colours was obtained. This Maxwell attempted to do by taking negatives of some ribbons through solutions of ferric thiocyanate, cupric chloride, and ammonio-copper sulphate. Positive transparencies from these were placed in three lanterns, illuminated by light filtered through the same solutions, and the images superposed upon the screen. This was shown at the Royal Institution in 1861, but, as the only photographic plates available at that time were but feebly sensitive to green and red, the result left much to be desired.

With the advent of orthochromatic plates this method was perfected by F. E. Ives, who used chiefly the Lumière C plate for taking all three negatives.

[Here a spectrum was projected and the absorptions of Ives' filters shown, an Ives' repeating camera back exhibited, and some photographs in colour projected by means of a triple lantern, the building up of the picture by means of the three coloured images being shown.]

Another process on the same principle, and which only involves the taking of one negative, consists in making a screen ruled in fine transparent lines of red, green and blue. This is placed in contact with a suitable pan-chromatic plate, a yellow filter being used if necessary in making the exposure. A positive transparency printed from such a negative will, when brought into

register with a similarly ruled screen, show the colours of the original. A disadvantage of the process is that about two-thirds of the light is lost by absorption in the screen, and of course no colour detail can be reproduced which is smaller than the width of a set of three rulings.

This method was devised and used by L. D. du Hauron in 1869, and was re-discovered by Professor Joly, who placed it on the market as a working process. There is also an American claimant to the honours of this process, named McDonough.

Joly's screens were mostly ruled 200 lines to the inch, but he succeeded in getting as many as 1,000 rulings to the inch. In this, as in the previous process, the taking filters embraced broader bands of the spectrum than the viewing filters.

[A magnified image of a Joly screen was projected by means of a microscope, and some specimens exhibited.]

About 1869, L. D. du Hauron suggested that, if the three positive prints were made not in black but each in the complementary colour to that of its taking screen, then the coloured result could be obtained by superposing these on each other. Let us examine this a little more closely. Suppose that we are copying a black and white diagram. A photographic negative will give us, by its silver deposit, a record of the distribution of white, and in printing from that we print in black, from the clear parts of the negative, parts corresponding to an absence of white in the diagram. If we now consider, say, a red negative out of a set, the silver deposit here gives us the distribution of red in the original, and, in printing from this, we shall print upon the parts where red should be absent, so that our printing colour should contain no red, but should be a colour which consists of white less red, *i.e.*, a greenish blue. In the same way the printing colours for the green and blue negatives are arrived at by subtracting these colours from white and are respectively a crimson (containing red and blue) and yellow. In fact, each printing colour, if it could be added to its filter colour, should build up a complete spectrum.

Further than this, if we consider the effects of over-lapping of the three records, we find that, where the red and green records overlap, the colour is reproduced as yellow, and where the green and blue overlap it is reproduced as greenish blue; so that the overlapping should be confined to those portions of the spectrum which

accord in tint with the printing colours of the red and blue negatives.

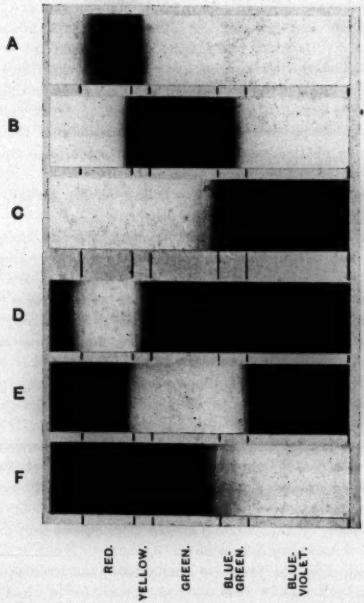
The change in sensibility of a photo-plate in passing through the blue-green makes it a trifle difficult to ensure proper overlapping in this region. In fact, most commercial tri-colour filters, when used with the plates for which they are intended, do not overlap at all in the blue-green, but very considerably in the yellow. This latter is a very bad defect, since it causes the greens to be degraded with red, and also sometimes impairs the reproduction of the reds by giving them an orange tint. The reproduction of yellow presents no difficulty.

Upon some of these points my colleague, Mr. Jolley, and I, will shortly be making a communication.

The reproduction of green is a very real difficulty, quite apart from the use of improper filters. Most orthochromatic plates have a band of comparative insensibility in the middle of the green, being generally more sensitive to yellow-green than to blue-green. The result of this is that the greens in any subject are not fully rendered, and any red that may happen to be in a green shade is relatively enhanced in the final result. Another factor is that it is practically impossible to find any pigment quite suitable for the blue printing. Nearly all blues and blue-green dyes transmit a little red, but this may be small; what is more serious is that they absorb a gradually increasing amount of light as one passes from the violet to the yellow, so that the dye may be very transparent to blue but yet absorb too much green.

This difficulty of reproducing greens often means that a certain amount of re-touching is necessary.

With regard to the processes themselves that have been used for obtaining prints by this method, some were worked out by Du Hauron and Du Cros about 1870, in which bichromated gelatine reliefs were used. The first process to be very successful was the somewhat complex one of Messrs. Lumière, of Lyons. The main features of this process are that contact prints were made from each negative on to bichromated gelatine containing a little pigment to prevent undue steepness. Each print was transferred to a temporary support, and developed with hot water as in the familiar carbon process. By this means positives were obtained in clear insoluble gelatine reliefs, in which



The above rough diagram illustrates how a spectrum is reproduced. A, B, and C are the photographic (negative) records through the red, green and blue filters respectively. D is a positive print from A made in greenish-blue, E is a print from B in crimson, and F a print from C in yellow. G indicates the colours produced by the superposition of D, E, and F. In the first space the crimson and yellow printing colours are superposed, and the common colour transmitted is red, blue being absorbed by the yellow, and green by the crimson. In a similar manner the common colour transmitted in the third space where the yellow and green-blue are superposed is green, and in the fifth space all is absorbed except the blue and violet. In the second space, corresponding to the region where the red and green negative records overlap, the colour reproduced is the printing colour of the blue negative, viz., yellow. Similarly in the fourth space we have the printing colour of the red negative, blue-green. All these colours should shade into each other.

density was represented by the thickness of the gelatine. These were then dyed with suitable dyes—erythrosine, diamine blue, and chrysophenine—to the required depth, and then stripped into contact with each other on a temporary paper support, and finally stripped on to glass and viewed as a transparency.

The method used by Lumière to reduce when necessary the depth of the dye in a positive is interesting in view of later developments. They soak the print for a short time in a weak solution of glue or very soft gelatine, when the dye is gradually transferred from the hard insoluble gelatine of the print to the glue in solution.

[Some specimens of Messrs. Lumière's work, kindly lent by the Lumière North American Company, were then shown on the screen.]

The next step forward in the matter of simplifying the process was made when F. E. Ives used ordinary celluloid films with a silver bromide emulsion which he sensitised in bichromate of potash. Here the silver bromide served the double purpose of preventing steepness and of serving as an indicator for the printing. A slight printed out image in silver being obtained, from which the progress of the exposure for the image in insoluble gelatine could be judged. The printing was done through the celluloid, so that transferring the film became unnecessary.

A transparency process in which the red and yellow prints are obtained in this way, and the blue print is made of a blue-toned lantern slide, is used by Mr. Sanger Shepherd.

[Some specimens of this process, kindly lent by Mr. Shepherd, were exhibited.]

More recently the Lumière N. A. Company have put forward a process in which all three prints are made upon a film in which mica replaces celluloid as a support. It has the advantage of being very thin, and not being affected by the heat of the lantern. On the other hand, it is only available for small sizes.

[Some specimens of this process were also exhibited.]

With regard to obtaining the prints upon paper, Messrs. Lumière have been able to do this by stripping their superposed films on to paper instead of glass, but recently two processes have been worked out which are more suitable for the ordinary Photographer. In the first of these the chromated gelatine positives are made and dyed in the usual manner. If now one of these,

while wet, is brought into contact with a film of soluble gelatine the dye will be almost wholly transferred to it with little or no spreading. The dye does not take many minutes to transfer, and, by absorbing all three successively on to a piece of gelatine-coated paper, a colour print is obtained. A better plan is to make the blue print on a bromide opal, a lantern plate, or a piece of glossy bromide soaked in water to allow it to stretch; then tone this blue, and absorb the red and yellow on to it afterwards. This process has been discovered independently by several people. Among these may be mentioned Dr. Bartlett, who dropped by accident a wet print on a piece of blotting paper and found that the image was transferred when it was picked up after a few minutes, and so was led to work out the method; and Mr. Stedman, of the Lumière N. A. Company, who also discovered the method while experimenting with paper prints early last November. The process is now worked as a patent one by Messrs. Sanger Shepherd and Company.

Another paper process, recently introduced by the Lumière N. A. Company, and which yields very good results, consists in making the red and yellow prints upon a stripping film, and the blue print upon bromide paper which is toned blue. The bromide paper has to be well soaked in water, and printed in wet contact with the negative. The red and yellow prints when adjusted to the right depth of colour are successively stripped on to the blue print.

[Some samples of this process were projected on the screen by means of Zeiss' epidioscope.]

The PRESIDENT said he was sure they all felt very much indebted to Mr. Bull, who had come forward at short notice to fill an unexpected gap. There was no subject—and perhaps there had not been for years past—that was so fascinating from a scientific point of view as the subject of colour photography. Apart from the fact that it was of great commercial importance, there were few things by which our attention was drawn to so many interesting scientific facts as by colour photography. A person to succeed in this branch of research must have almost every qualification as a physicist and as a chemist also. Referring to a matter in connection with the history of the Northampton Institute, he might say that he (the President) had thought some years ago that the problems of colour photography offered

an excellent field for investigation there. However, their principal demonstrator in electrical engineering had left them, and he (the speaker) found it necessary to devote much more attention to that department. Fortunately, Mr. Bull had since joined them, and he was well equipped, as an excellent physicist and chemist, for dealing with the complexities of colour photography. Some of the results those present were now able to see. Dr. Drysdale proceeded to express his faith in the future development of colour photography. Three-colour printing, as used in the production of magazines with which they were all familiar, had not been touched on by the lecturer. The photo-mechanical printing plates that were produced (apart from hand work) for such three-colour reproductions often bore a very slight resemblance to the original from which they were photographed. Retouching or fine etching —a great aid to the artistic craftsman—was brought very much in evidence. The method ought, however, to be made a scientific one, and hand work should be got rid of while retaining a fair degree of accuracy of reproduction. Satisfactory dyes could be procured for making coloured transparencies, more easily than the equivalent printing inks; and those inks which had proved suitable for the purpose in other ways, were somewhat fugitive. This might be said particularly of the reds. Having referred to the great scientific beauty of the Lippmann process, Dr. Drysdale concluded by asking a hearty vote of thanks to the lecturer—whose own handiwork they had seen in some of the remarkable coloured prints on paper that had been exhibited. Their thanks were also due to Mr. Merinier, for his labours during the evening in the manipulation of no less than four lanterns.

Mr. A. E. Grey said that the lecture was one of the most interesting it had been his privilege to listen to as a member of the Society; and he had no doubt the subject would appeal equally to most of those present. He had very great pleasure in seconding the vote of thanks, and would put it to the meeting. The vote was carried by acclamation.

Mr. Bull, in acknowledging the vote, expressed his general agreement with the President's remarks on the fine-etching of process blocks. He proceeded, however, to throw on the screen one remarkably successful three-colour print which had been made, by purely photographic methods, by Mr. Newton, of the London County Council School of Lithography.

SOME PROPERTIES OF GLASS.

PAPER BY MR. WALTER ROSENHAIN, B.A. (CANTAB.), B.C.E. (MELBOURNE), *Member of Council.*

**Read before the Optical Society on June 15th, 1903.*

THE object of the present paper is to summarise in brief form certain comparatively well-known facts concerning those physical and chemical properties of glass which are of special importance in connection with all classes of optical instruments. For the present purpose, I propose to exclude those properties particularly known as "optical" which are expressed by the constants of refraction and dispersion. These are of vital importance, but they constitute a very large subject in themselves, and for that reason demand separate treatment. The subjects with which this paper is to deal are principally colour and transparency, hardness and durability, thermal expansion and internal strain.

In the majority of its optical uses, the most essential property of glass is its transparency; the only exceptions to this rule are those cases where the glass is merely used to carry a reflecting film of silver or other metal—in these cases the reason for the use of glass lies in the hardness of its surface, its power of readily acquiring and retaining a brilliant polish, and—as compared with most metals—its light specific weight. Nevertheless, in the great majority of optical instruments, glass acts as a transmitter of light. Unfortunately, very little is known as to the mechanism by which transparent substances are enabled to transmit light. Modern experience of electric waves is tending to confirm the view that waves in the ether can, as a rule, traverse the space occupied by matter without suffering more than a certain retardation; that, in fact, in this sense transparency is an almost universal property of matter, with this important reservation: that for each kind of matter, the power of allowing

* Owing to the indisposition of the author, the paper was read for him by Mr. S. D. Chalmers, M.A.

ether waves to pass is restricted to waves of a certain limited range of length. The bodies which are ordinarily called transparent are those which have the power of transmitting waves of that particular length which is capable of affecting the eye and of producing the sensation of light. The kind of waves which a given substance is capable of transmitting, or—what comes to the same thing—the kind of waves it is capable of absorbing, depends upon the size and weight and arrangement of the molecules, and, perhaps most of all, upon the electric charges carried by them. In the simplest class of bodies—gases—we know that a substance will absorb the same kind of light as it is itself capable of emitting when hot; and, no doubt, the absorption of light by solid bodies is governed by somewhat similar principles, only that the greater complexity of the conditions has so far prevented us from tracing the details of the phenomena. There is, however, a physical constant which can be determined for every substance, known as the "specific inductive capacity," and the electro-magnetic theory of light teaches that the refractive index of a substance should be equal to the square root of this specific inductive capacity. But this is only the roughest of first approximations, and experiment does not really confirm it. In measuring specific inductive capacity in the ordinary way we are doing something analagous to measuring the tensile strength of a piece of metal by the application of a steady load; now, it is well known that in the case of the metal we shall obtain very different results if the load be applied with a sudden jerk, and still more so if it were to be applied and reversed many thousand times per second. But in comparing the electric actions which go on when a steady electric stress is applied, as in the usual method of measuring specific inductive capacity, with those which go on when a light wave of many thousand reversals per second is passing through the medium, we are as unlikely to arrive at correct conclusions as in the somewhat analogous case of the metal. We might express this in another way by saying that theory indicates that there is a very intimate connection between the behaviour of a substance as an electric insulator and the velocity with which light-waves will travel through it; but that its insulating properties must be considered not when a steady electric pressure is applied to it, but when that electric pressure undergoes as rapid a series of alternations as the vibrations of

the light we are considering. Somewhat on these lines, Paul Drude has formulated a very beautiful theory of optical dispersion, but I cannot go further into this fascinating subject here; I shall have to refer to it again when dealing with the question of the colour of glasses. As regards transparency, we can say little more than that it is associated with certain properties of electric insulators; all good conductors of electricity, such as metals, are opaque to light except in the thinnest of layers.

Transparency, to light waves, however, is not a very common property of matter, and it is interesting to consider to what classes transparent bodies belong. We find that they are either

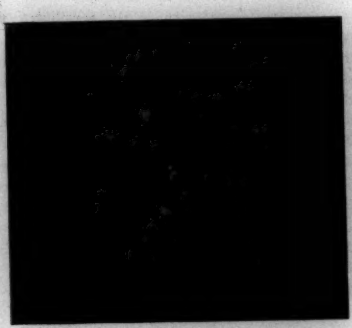


FIG. 1.

homogeneous fluids or homogeneous crystals, and the question at once arises—does glass belong to either of these classes? I have no hesitation in expressing the view that glass is not a crystalline body in its ordinary state, as it presents none of the characteristics of that class of bodies. Under certain circumstances glass may become crystalline, but its entire properties are then altered, transparency entirely disappearing. This is a phenomenon known as "devitrification," and when kept at certain critical temperatures almost any kind of glass will undergo this

change, which I regard as the crystallising out into definite crystalline compounds of some or all of the substances present in the glass in solution or mixture. The change begins, like all crystallisations, simultaneously at a number of points throughout the mass of the glass, more or less spherolitic crystals being formed, which ultimately meet, thus occupying the entire mass. The photograph, fig. 1, shows a specimen of glass in which this process has been arrested, by sudden cooling, at an intermediate stage. The crystals in this case are Wollastonite. This property of crystallisation points to the view that glass is in reality a fluid, and owes its apparent solidity to its great viscosity. This view is borne out by several other lines of evidence, the most important of which is that glass has no definite melting or freezing point, but passes continuously from an obviously liquid into an apparently solid state, and *vice versâ*. In a general way this fact has

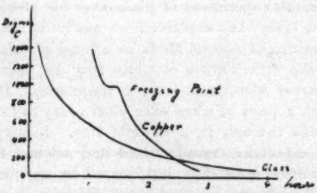


FIG. 2.

been known as long as glass itself, but I thought it worth while to apply the method used with such success in the study of steel and other alloys, and to take a pyrometric cooling-curve. A small crucible of glass is raised to a white heat, the thermo-couple inserted and attached to a recording pyrometer, and the whole allowed to cool. The pyrometer curve represents temperature as ordinate with time as abscissa, and is reproduced in fig. 2. As will be seen, the curve is quite smooth, thus proving that no sudden solidification—or, what is the same thing, crystallisation—can have taken place. For the sake of comparison, the cooling curve of copper, with its well-marked freezing point at about 1,050 C., is also shown in fig. 2. I have gone into this subject

somewhat fully because two English chemists have recently advanced the view that glass is a crystalline aggregate, a view I would most strongly contest. These Chemists, Messrs. Jackson and Rich,* illustrate in their paper some micro-photographs of radiating, more or less crystalline figures revealed on the surface of certain glasses by means of etching with hydrofluoric acid, but they state that these figures could only be obtained on surfaces which had been obtained by "fire polish," *i.e.*, had solidified thus from fusion, but could not be obtained on polished sections.† I am inclined to regard these figures as truly indicating the existence of rudimentary crystal-skeletons in the surface, but to attribute their presence there to an incipient devitrification, which is very apt to occur—if it does not always occur more or less—on fire-polished surfaces. Messrs. Jackson and Rich have also succeeded in separating constituents of different density from certain dense flint glasses, but this is, I think, simply a proof that the particular specimens of glass were not absolutely homogenous, which may be explained as easily by supposing an imperfect mixture of viscous fluids as a mass of mixed crystals.

Whatever the true nature of glass may be, it can only be truly transparent when it is truly homogeneous. It is possible to see through a piece of glass only when every ray of light that enters it passes through in a straight line. In very good and perfect glass especially manufactured and selected to this end, the above condition is satisfied; but it is not by any means strictly satisfied in all ordinary kinds of glass. If we take an ordinary piece of plate-glass, we find that it is really very far from homogeneous. Looking through it in the ordinary way, it is very difficult, with the unaided eye, to detect any want of uniformity, but it becomes apparent when a greater thickness of glass is looked through. For this purpose it is only necessary to polish two opposite edges of a plate a few inches wide; it is as a rule hardly possible to see through the glass in this way at all. The defect exhibited by glass of this kind is known as "string" or "striæ," or "veins," and is no doubt due to imperfect mixing of the contents of the crucible. A very similar appearance is

* W. Jackson, A.R.C.S., and E. M. Rich, B.Sc.: "The Constitution of Glass," Journ. Soc. Chem. Ind. 1901. 555-560.

† This result had been obtained by Leydolt in France, long ago. See Compt. Rend. 34, 565.

produced whenever two miscible fluids of different density are first stirred up together; for instance, if some pure water be poured upon a dense sugar solution and stirred a little, the fluid will appear full of these striæ, and they will only disappear on prolonged stirring. This defect is at once the most troublesome, frequent, and serious of all those to which glass for optical uses is liable. The veins are often so fine that even an experienced eye fails to detect them, and they are only discovered when a finished lens gives bad definition or multiple images. There are, however, methods of examining glass by which even the minutest striæ may be detected. The simplest of these is to hold the glass up between the eye and a small gas-flame, and to examine the glass through a small magnifying glass of low power, held in such a way as to be focused upon a point within the glass, which will then appear bright, any veins being readily detected as

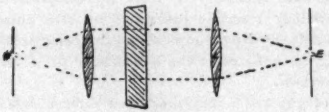


FIG. 3.

shadows. A more perfect arrangement for the same purpose is illustrated in diagram, fig. 3. Two similar simple double-convex lenses of about 6 in. focus and 3 in. diameter are set up on a common axis, about 6 in. apart. At the principal focus of one a small source of light is placed—such as a pin-hole in a screen, with a light behind it. The first lens converts the light diverging from this point into an approximately parallel beam, which the second lens converges to its own principal focus, where the eye is placed. To the eye so placed the whole surface of the nearest lens appears brightly and uniformly illuminated. If now the piece of glass to be examined be placed between the two lenses, and this glass be truly homogeneous, no change will be produced; but if the glass contain any striæ, these will at once appear as dark lines in the field, and their course can be easily marked with a suitable pencil.

The transparency of glass is sometimes impaired by a want of homogeneity of a totally different kind, arising from the presence of foreign bodies in the mass of the glass. These may be either solid particles of opaque matter or transparent bubbles of gas. The former defect is comparatively rare, and can be entirely avoided in all but the largest pieces of glass by suitably cutting away the contaminated portions. This applies especially to those cases in which the opaque substance is a foreign body which has found its way into the glass during the process of manufacture. In some cases, however, this opaque material is not of foreign origin, but arises from crystallisation occurring within the glass itself. I have already referred in some detail to this process of crystallisation or devitrification, and it follows from what has been said that the facility with which it will take place will very much depend upon the chemical constitution of the glass. Now, in order to meet modern optical requirements it becomes necessary to force the composition of certain glasses until they resemble saturated, or even super-saturated, solutions, ready to deposit crystals at the least provocation; the result being in some cases the production of a very beautiful opal or porcelain.

Bubbles of gas are a very much less serious defect, especially in optical glass, where the bubbles are small and of spherical shape, not distorted or elongated as in glass that has been blown. The only effect of the bubbles is to absorb a small proportion of light, and they do not at all interfere with the formation of the image. Moreover, in certain extreme glasses, their presence even in considerable numbers is quite unavoidable.

The transparency of glass is affected by yet another class of phenomena, viz.: those of absorption of light by the glass itself. I think it may fairly be said that the power of absorbing light is the most valuable optical property of glass. This statement is paradoxical at first sight, but is correct, because refraction and dispersion are direct consequences of absorption: if glass did not check and absorb a small portion of the light-waves reaching it, those that passed through could not be deflected. As a rule, the glass that has the highest refractive index, *i.e.*, which has the greatest power of deflecting light-waves, will also absorb the greatest proportion of the light that enters it. This is not strictly true, as some glasses contain substances having

specially absorbent properties, and also as the absorbing power of a glass is different for different regions of the spectrum, *i.e.*, for light of different colours. Further evidence on this question of absorption is, however, needed, as determinations have only been made on a few Jena glasses for the purpose of making a selection for the lens of a large telescope at Potsdam, and these measurements revealed the fact that while flint glass showed generally a stronger absorption for all colours than crown, yet the crown glasses showed a relatively greater absorption for the red rays, while the flint glasses showed a much stronger absorption for blue and violet rays. The electro-magnetic theory of dispersion, as enunciated by Paul Drude, explains this on the assumption that for each glass there are at least two wave-lengths of light—generally lying far beyond the visible range in the spectrum—for which the glass is entirely opaque, while its power of absorption increases up to this point as this wave-length is approached from either side. In certain glasses, which we call coloured, these "absorption bands" fall within the visible range; as, for instance, in a ruby glass, which only transmits red light, absorbing all the other colours. In other glasses the absorption band, although itself outside the visible range, is near to either end of the visible scale, and such a glass then shows comparatively strong absorption of light near that end of the spectrum, and its dispersion is also much greater towards that end than the other. The most marked instance of this kind is supplied by very dense flint glasses, which have a pale yellow colour, showing that their absorption-band lies near the blue end of the spectrum. Lighter flint glasses are colourless, or nearly so, when cold, but show the yellow colour when hot. Yet even in the cold, such glass when seen in large thicknesses shows a slight colour. There are a few special glasses which hardly show any colour at all even in very large pieces, but they are invariably of very low refractive index. Yet even in these glasses the existence and position of absorption bands outside the visible spectrum can be ascertained from a study of their dispersion, and can even in some cases be demonstrated. The well-known opacity of glass to long heat-waves from non-luminous bodies is an example of ultra-visual absorption. The point which is of special importance in the present connection, however, is that the colour and absorption and consequently the

dispersion and refraction of a glass are intimately connected; that the yellow tinge of very dense flint glasses is not a defect of manufacture but inherent in the constitution of the glass, and that almost perfect transparency is only to be looked for in glasses of low refractive index. When, therefore, the transmission of as large a proportion as possible of the incident light is one of the objects aimed at in the design of a lens, the glasses should be chosen of as low refractive indices as possible, so long, of course, as other considerations do not outweigh this one.

While dealing with the subject of colour and transparency, it is necessary to refer to certain other causes of colour and absorption which are not inherent in the nature of the glass, and arise from more or less avoidable contaminations introduced into the glass by the raw materials or during the manipulations of manufacture. These contaminating substances are themselves deeply coloured, and are present in the glass either in solution or in suspension. That they do not enter deeply into the composition of the glass is shown by the fact that the effect of their presence upon the refraction and dispersion of the glass is very slight. I am, therefore, inclined to regard them as mechanical admixtures in a state of colloidal solution, and I think that Zsigmondy's recent demonstration of diffraction effects arising from the presence of fine particles in certain ruby glasses proves the correctness of that view in those cases. There is, however, one interesting point of attachment between the colours due to inherent constitution and those due to foreign colouring matter; it is this—that the extent to which a given amount of colouring substance affects the colour of a glass is greater for denser, or more highly refracting glasses than for lighter glasses. In other words, as the refractive index of a glass increases, so the glass becomes more sensitive to colouring matter. Glasses of high refractive index are therefore more liable to be coloured by minute traces of impurity than glasses of low refraction, and as such impurities are never entirely absent, it follows that where extremely white glass is desired, a glass of low refraction must be chosen.

From the practical point of view I am inclined to think that two much importance is attached to the colour of glass. A faint tinge of green can hardly affect the image in a camera, for

instance, and even when a plate of glass appears distinctly green, the small thickness used for most lenses is hardly ever sufficient to allow the colour to be detected in the finished article. Now, in most cases, it is possible to cloak such a green colour in a glass by adding to the glass a minute quantity of a substance giving a complementary pink tinge, and thus the demand for a "colourless" glass is met. But what has really been done in such a case? The green tinge in the glass is, of course, due to the absorption of a certain proportion of the red rays, and to balance this absorption and so render the glass colourless, a substance has been introduced causing a corresponding absorption of blue rays, so that by requiring a glass to appear colourless we have diminished and not increased the total amount of light it transmits. I therefore feel justified in describing the demand for colourless glass, when pushed beyond a certain point, where the colour would become apparent to the eye, as a prejudice.

A further point remains to be considered in connection with the colour of glass, and that is the permanency of such colour. It is a well-established fact that certain glasses change their colour under the action of sunlight, although one finds it difficult to believe that so stable and apparently inert a substance as glass should be liable to a class of change which we usually associate with the delicate and unstable substances dealt with by the Photographer. Evidence of colour changes in glass is, however, plentiful. In the glazed roofs of some large buildings, such as railway stations—when they are clean enough to be examined—it will be seen that some of the panes show a yellow or even a pink tinge, and there can be no doubt that originally these were of the same faintly greenish tinge as the others. Many instances of this kind are to be found in the window-panes of some of the older houses in London; a very prominent example is a window at No. 4, Marble Arch, which has a very decided pink tinge. If it be urged that in these cases the glass may have been pink to begin with, I am in a position to say that I have seen glass of known history which has undergone the same change.

A large number of experiments have been made upon this subject, those who have dealt with it including Faraday, Pelouze, Fresnel, Bontemps, Melloni and Hunt, and Gaffield. The

latter has carried out a very elaborate series of tests of all manner of glasses on the roof of his house in Boston, some of the glasses being exposed for over 13 years.* He found changes of colour in what may be termed "white" glasses, as follows:—From white to light yellow, light green to yellowish-green, brown and greenish-yellow to various tints of purple, light green to light blue; and, in some cases, merely a deepening of the original tint. In the majority of coloured glasses, he found little alteration, except in violet and brown tints. Nevertheless when ancient coloured glass is removed from its setting, it is often found that the portions of the glass which have been protected from the light are of a different tint from those portions which have been freely exposed, so that there can be little doubt that we do not now see these ancient stained glass windows with the colours they originally possessed, and it is curious to enquire how far these windows owe their matchless beauty of colour and softness of tone to the long-continued action of light.

Another and very important example of the sensitiveness of glass to light has been pointed out by Sir Benjamin Stone in connection with photographic plates; if a developed plate be cleaned off and re-coated and then again used, prints from the second negative will sometimes show traces of the image of the first negative, thus causing the mysterious appearance of "ghosts" on the photograph. Apparently some change has been caused in the glass itself by the action of light, either during exposure and development, or—in my opinion, more probably—in the printing process.

It is difficult to offer any reasonable explanation of these phenomena of photo-sensitiveness in glass. The only theory which has come to my knowledge attributes the change in colour to a change in the state of oxidation of the manganese and iron present in the glass, and evidence for this supposition is found in the fact that the pink tinge sometimes assumed by the glass resembles the pink tinges which can be imparted to glass in the first instance by the addition of a rather larger quantity of manganese in the form of peroxide. Further, all the glasses of which I have records of colour changes, contain either manganese and iron, or lead, or all three. A recent

* American Association for the Advancement of Science, Boston, 1880.

discovery bears upon this point. It has been found that the rays from radium have the power of changing the colours of certain glasses.

For the optician, the uses of coloured glass are almost exclusively those of light-filters in photography, and I need not enter into the details of the choice of glasses for special purposes. It is obvious how conveniently the spectroscope may be used as a guide in such questions, especially when used so as to record its results photographically. Unfortunately, however, it is often very difficult to find a glass of the right colour, and, what is equally important, of the right *intensity* of colour. In very thin sheets, coloured glass will often transmit rays which are entirely cut off by a greater thickness. The importance of paying attention to this point becomes evident when we consider that in a great many coloured glasses, the sheet of actual coloured substance is very thin indeed. These are the so-called "flashed glasses," where a sheet of white glass simply bears a thin film or veneer of deeply-coloured glass on the surface. Such flashed glasses may be readily distinguished from glasses coloured throughout their mass by looking through a small piece edgewise. The "flashed" glass will appear white or light green, while the solid coloured glass will appear either black or deeply-coloured with its proper colour.

In considering the changes which the colour of glass is liable to undergo, I have dealt with one class of instability to which glass is subject; I come now to deal with another class of changes which glass may undergo, viz., alterations of surface, or, rather, alterations commencing at the surface. This is a subject upon which both glassmakers, physicists and chemists have spent much time and money in research and experiment, with the result, however, that the causes and nature of these changes are now fairly thoroughly understood. I can only attempt to summarise the most important points.

The surface of glass is liable to deterioration in several ways. Of these, probably the most frequent and serious is direct physical injury, generally in the form of scratches. Those accustomed to handle finished glass are doubtless aware of the precautions requisite to avoid such injury, but I do not think that the users of spectacles, for instance, are aware of them; at all events, they frequently neglect them. For the present purpose, however, the important point is this, that glasses vary very much in their

liability to scratching. This is due to two causes. One is that glasses differ very much in mechanical hardness, some being almost as hard as quartz, while some of the heavy flint glasses are very soft indeed. It is obvious that the harder glass will in the course of inevitable friction with surrounding objects, meet a smaller number of particles capable of scratching its surface, so that with equal care, or want of care, the harder glass will preserve its surface longest.

To test the relative hardness of various glasses is a fairly simple matter, although it is by no means easy to obtain quantitative results. For comparison purposes, the simple test of scratching a series of surfaces of known hardness is sufficient, or, if it be more convenient, a sheet of the glass to be tested may be scratched with specimens of known hardness. In examining the hardness of glasses by scratching tests, it will be found that when considerable pressures are used every kind of glass will scratch every other kind more or less, and the character of the scratch must be examined by the aid of the microscope and taken into account in determining the relative hardness. No satisfactory scale of hardness for this purpose is in existence, the mineralogists' scale (Moh's) not being suitable, as most varieties of glass fall between two or three of its numbers. The establishment of a standard scale and method of testing the hardness of glass is very desirable. The scratching test alone, however, will not give true results unless the character of the surface tested be taken into account; for instance, the fire-polished surface of a piece of blown glass will have a different hardness from a polished surface of the same glass, while the state of annealing of the glass must also be taken into account. A diamond which will cut annealed sheet-glass with perfect ease will hardly touch the surface of the same glass as it comes from the blower before being annealed thoroughly.

Two other methods of testing hardness which have both been applied to glass should perhaps be mentioned here. One of these, which has been applied to a series of Jena glasses by Auerbach in a very complete manner, depends upon the observation of the area of contact between a spherical and a plane surface of the same glass under a known pressure. But, unfortunately, it is found that the hardness as determined by scratching experiments is not the same as that found by these

indentation tests. For the Optician, however, it is the power of resisting mechanical injury that is of first importance, and mechanical injury is much more likely to be caused by scratches than by indentation under pressure. The other method of testing "hardness" is that of M. Jannetaz et Goldberg, and really consists in simultaneously exposing various samples of the glasses to be tested, along with a sample of standard hardness, to a steady grinding action, as on a rotating disc fed with emery. The results of tests made in this manner are of greater practical value than those obtained by indentation, but they do not in all cases coincide with the results of scratching tests.

Apart from durability of surface, the hardness of a glass has interest for the maker of optical instruments from the point of view of the ease and cost of cutting, grinding, and polishing. I think it will be generally admitted that the harder glass will grind and cut more slowly, but that it will take a better polish and be less liable to alterations of shape through slight unevenness of rubbing in the polishing stage, so that probably even from the point of view of cost in working, the advantage will lie with the harder glass, at all events in those cases where accurate figuring is required.

To return to the question of the durability of glass surfaces, we now come to a source of injury which lies midway between the purely physical injury due to scratching and the purely chemical injury due to weathering. Some glasses, on prolonged exposure to the air, become coated with a very thin film of decomposition products, and these are in some cases in the form of hard, sharp crystals. If a glass so coated be wiped with a dry cloth, the sharp angles of these crystals are dragged about over the surface, which becomes badly marked. As a rule, these crystalline deposits are soluble in water, so that if the glass be first washed with water, this class of injury may be avoided. In good glasses it is only liable to arise if a lens has been put away for a long time and is then taken out and cleaned.

Before entering upon the consideration of the chemical deterioration of glass surfaces, it may be well to point out the great importance of the question. In optical instruments, the chemical deterioration of the glass surfaces may be a very serious matter. To take an example, it is evident that the surfaces of the prisms in the modern prismatic binocular should

possess very great stability; in many cases they are inaccessible for cleaning except to the expert, and even for him it is a delicate matter. This becomes especially important where the binocular is intended for use at sea or in damp climates, so that extreme care in the choice of glass is necessary in this case. Fortunately, the requirements can be fully met, as no special demands need be made on the optical constants. The hard crown and borosilicate crown glasses commonly used for this purpose can be obtained of such a quality as to remain bright under all ordinary conditions. Examples of cases where great care in the choice of glasses from this point of view is necessary might be multiplied almost indefinitely; the outer lens of telescope object-glasses, and the lenses of eye-pieces nearest the eye, are particularly important examples.

In some of these cases, and especially in modern photographic objectives, demands are made upon the optical constants which render it very difficult, if not impossible, to comply with the conditions for great chemical stability. It is a deplorable fact that even the great Jena firm have been compelled to abandon a large number of glasses having most desirable optical properties, because of their chemical instability. The chemical details of this subject are beyond the scope of the present paper; those interested will find further details on this and some of the other points dealt with in a book by Dr. H. Hovestadt, recently translated into English—and considerably improved in the process—by Professor and Miss Everett. The only point upon which I can dwell now, is that as a general rule extreme optical constants can only be obtained by means of extreme composition, and extreme composition always involves a risk of instability. It has been shown that high percentages of alkali, lead, boric, or phosphoric acid, tend to render a glass less stable chemically; and yet mathematical Opticians clamour for optical constants which can only be obtained by the use of these substances in proportions greater than the limit suggested by chemical considerations.

Another aspect of chemical instability of glass surfaces is worthy of consideration, namely, in connection with the handling of the glass in the making of optical instruments. Cases have come under my notice where glass has been injured in this way, chiefly in the course of heating required for cementing lenses

together. In one case flint glass was found to show minute yellow specks; when, on my suggestion, the lenses were heated over a spirit flame in place of the gas flame formerly employed, the specks no longer appeared, and there is no doubt that the defects were caused by the sulphur contents of the gas, especially as it is known that sulphurous gases will strongly attack glass when hot, especially in an oxidising atmosphere.

Having realised the importance of the chemical stability of glass surfaces, it becomes desirable to find some ready test whereby the quality of a given glass in this respect may be determined. Where fairly good glasses are concerned, it is too slow a process to keep samples and to note the time in which the first signs of weathering begin to appear. Still, as a final test, this is almost the only one available, so that it is in all cases a desirable practice to keep dated samples of all glasses used. Some caution is required, however, in the manner of keeping such samples. They should be kept free from any paper or other wrapping, laid on a shelf in a cupboard or other receptacle as free from dust as possible, and, when examined, they should only be touched at the edges, and not wiped in any way. The absence of dust is very important, because it is found that the chemical changes in glass surfaces always begin at dust particles, and it seems not at all unlikely that certain bacteria may play an important part in the disintegration of the glass. It is hardly necessary to say that all corrosive fumes should be excluded from the samples of glass, unless it is intended to test the power of the glass to resist such fumes—a test which is hardly ever required.

Prompter methods of arriving at a conclusion as to the durability of the surface of a glass are, however, available. These are—

(1) Chemical analysis.

(2) Exposure to the vapour of hydrochloric acid.

(3) Exposure to an ether-water solution of iodeosin.

The application of all of these tests requires a certain amount of laboratory equipment and an operator accustomed to chemical manipulation, and these are frequently not at the disposal of the user of optical glass. The testing of glass by such methods seems, therefore, to be an eminently suitable task for the

National Physical Laboratory, and that institution will no doubt undertake such tests when they are required.

The first of these methods—chemical analysis—does not lend itself to treatment in the present paper. For the ordinary silicate glasses the analysis is fairly simple, and when properly performed its results are satisfactory and reliable. The data obtained by this means—thanks to the elaborate researches of Weber and Mylius and Foerster, enable us to form a good judgment on the keeping qualities of the glass. But when more complicated glasses are dealt with, the actual results of the analysis become uncertain, and even a correct statement of the composition of the glass will not give a certain guide to the stability of the surface. A modified form of partial analysis, however, whereby the solubility of the glass in water is determined, is applicable in all cases, and it may be fairly assumed that the rate at which the glass will be attacked by atmospheric agencies will be proportional to the solubility of the glass in cold water. For such determination of solubility, the glass is first reduced to a powder of standard grain by pounding it, and then passing through two sieves. A known weight of this powder is then exposed to a certain volume of water and the amount dissolved is then determined.

The second method of testing stability of surface, by exposure to the vapour of hydrochloric acid, is due to Weber. It is carried out as follows:—A slip of the glass to be tested is exposed to the vapour of hydrochloric acid for 24 hours. This can be conveniently done by pouring some concentrated acid into a porcelain basin and laying the strip or strips of glass to be tested across the top of the basin, care being taken that the acid itself does not touch the glass. A bell-jar is then placed over the whole. At the end of 24 hours, the slips of glass are removed from the basin and placed to dry under another bell. The test slips must, of course, have polished surfaces to begin with, and when the drying is complete, these surfaces will be more or less coated with crystals of chlorides derived from the action of the acid vapours on the bases of the glass. The durability of the glass surfaces is to be estimated from the density of this film of chlorides, a process obviously requiring much experience, and the use of a standard for comparison. For a chemist it would, perhaps, be more satisfactory to dissolve the

film of chlorides in as small a quantity of water as possible and then to determine their quantity analytically.

The third method—by the use of iodeosin, is generally acknowledged as the best and most sensitive. Again, it is only a method of comparing the solubilities of glasses in water, but this time by using a very sensitive colour-reaction for the detection and estimation of the amount of alkali dissolved, and a standard for comparison must be exposed to the test reaction either at the same time as the sample to be tested, or else the standard tint must be carefully preserved for reference. The samples for this test should preferably be of the shape of thin slips, polished on all sides; they must be carefully freed from any grease or rouge adhering from the polishing process, then soaked for a short time in water, then washed in spirits until all the water is removed, and then washed in ether and finally immersed in the test solution while still moist with ether. They are left in contact with the test solution for 24 hours, then rinsed with ether and allowed to dry. The surface of the glass will be coloured more or less pink, and the depth of the colour measures the extent to which the glass is subject to attack. The test solution is made up by saturating 100 c.cm. of ether with water, and then dissolving 0.1 gramme of iodeosin in the aqueous ether. This solution must be kept in well-stoppered bottles, which it will, of course, stain pink in time.

I have postponed the consideration of the thermal properties of glass to the end of this paper because they are not of very direct importance to the users of glass for optical purposes. In optical instruments we have rarely to deal with masses of glass large enough to involve risk from unequal expansion. Only where larger lenses are cemented together and the combination is liable to be exposed to tropical sunshine does a knowledge of the coefficients of expansion of the two glasses become of importance. If the glass is available in the shape of tubes, the expansion may be readily measured by the well-known method of the weight thermometer, and this might be extended to cases where only a rod or other solid piece is available by using two weight thermometers both of one kind of glass, but one of them enclosing the rod or piece of the glass to be tested. The ordinary weight thermometer is then used to find the expansion of the tube glass, and the expansion of the rod can be calculated from

the behaviour of the weight thermometer containing it. This, however, is not a very desirable method, owing to its indirectness and probable inaccuracy. Where, therefore, the glass is only available in solid pieces, one of the modern methods for measuring expansion by the wave-length of light, such as the Abbe-Fizeau method, should be used. This, again, would seem to be, in those cases where the importance of the result justified the measurement, a fit task for the National Laboratory.

The relative expansions of glass and the metal in which it is held are of more frequent importance. As a rule, difficulties in this direction are met by allowing room for expansion, but, where this is not possible, the use of a nickel-steel, having the same coefficient of expansion as ordinary glass, is indicated.

The thermal properties of glass as affecting thermometers, gauge glasses and lamp chimneys, though very interesting, are

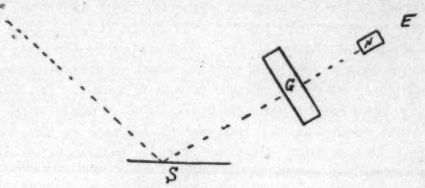


FIG. 4.

foreign to the subject of the present paper. There is, however, one way in which the thermal properties of glass do affect optical instruments very materially, and that is by producing, under certain circumstances, severe internal strain or even fracture. Glass is an exceedingly bad conductor of heat, with the result that when one portion of a large mass of glass is suddenly heated or cooled, it expands, or contracts, long before conduction can have raised the temperature of the adjacent parts to the same degree. If this expansion is greater than the strength and elasticity of the material can withstand, fracture occurs. Consequently very great care is required in heating or cooling glass in large masses. When glass solidifies from fusion and is allowed to cool with any sensible rapidity, the outer layers of the mass become cold and hard while the inner portions are still soft and hot. When, subsequently, the inner layers also

cool, they are prevented from contracting by the solid outer layers, and the interior of the mass solidifies under great tension. A familiar case is the Rupert's Drop, which flies into fragments when the outer skin is ruptured. In a much milder degree such internal stress is a very common defect of glass. For many optical purposes it is of very little importance, particularly when the strain is symmetrical, as then the only effect is to slightly alter the effective refraction of the glass. In many cases, however, internal strain would practically destroy the homogeneity of the glass and render it useless, at the same time causing a serious risk of breakage when the glass is cut or ground. The defect is readily detected by the fact that strained glass has a polarizing effect on light. A simple method of testing a specimen of glass is indicated in fig. 4. S is a sheet of black glass, or a smooth board painted black, so placed that the light from a lamp or window is reflected into the eye at about the polarising angle. E indicates the position of the eye, G that of the glass to be tested, while N is a Nicol's prism. The glass must, of course, be provided with suitable polished faces to enable the light to pass through. A strained piece of glass will show coloured patches, or even a regular pattern and cross when examined in this way. Ordinary well-annealed glass will show only a few faint shadows, while perfectly-annealed glass will show no special effects whatever. This last result is only attainable by cooling the glass at an astonishingly slow rate, but a good deal depends upon the shape of the object. Corners almost invariably cool a little faster than the bulk of the mass, thus introducing minute strains, so that where the highest degree of perfection is required, the glass should be moulded and annealed in a cylindrical shape, and the lens or block cut from that shape.

I have now endeavoured to summarise as briefly and in as practical a manner as possible some of the leading facts concerning the properties of glass with which it is of importance for the user of glass for optical purposes to be familiar; in doing so I must have repeated what is, no doubt, familiar to many of those to whom this paper is addressed, nor can this paper claim to be in any sense a complete treatise on the subject; I claim for it only the humble usefulness of a convenient resumé of the leading facts.

Professor Everett said Mr. Rosenhain had expressed the opinion that glass was not a crystalline body in its ordinary state, and that raised the important question, What then is it? He was reminded of Graham's division of bodies into the two classes, crystalloid and colloid. Glass seemed to have a considerable resemblance to the colloid bodies, both in respect to its physical properties and also to its optical property of transparency. A very scientific suggestion had been made, and one that the speaker highly approved of, in respect to the mischief that results from trying to do good by the association of two evils which do not kill one another—the production of a "colourless" glass by the addition to greenish glass of a minute quantity of some substance giving the complementary pink tinge. It was again an interesting point that had been advanced as to the fine, rich tints seen in old stained glass windows. The suggestion that Father Time had had a hand in the production of these effects was very interesting. It was a strange fact that a diamond would hardly "touch" the surface of unannealed glass. The outer skin, Professor Everett supposed, must be in a very dense state. There must be great tension between the outer and inner layers—the inner substance (that which is the last to solidify as the glass cools) exerting a great pull. Glass, it might be mentioned, had about 15 times more resistance to "squeeze" than it had to "pull"; and in this case the strain in the skin of the glass was of the nature of a thrust, like that in a rod under longitudinal stress. A very practical suggestion had been made as to the use of nickel steel in connection with glass work. Physicists had been in the habit of saying that platinum was the only metal possessing nearly the same coefficient of expansion as glass, and available for making the metallic connections in vacuum tubes. Platinum was always made use of for this purpose, and it was said that a wire connection of any other metal would shrink away from the glass. Glass was the best material for making *x*-ray tubes; but, of course, there were objections to the use of platinum. He (the speaker) would like to ask Mr. Rosenhain if nickel steel would be a satisfactory substitute for the noble metal. Another question he would put, with reference to the colour that was so apt to appear in flint glass lenses. He had often heard that silver, present as an impurity in the lead which was one of the con-

stituents of such glass, was the cause of the objectionable colour. Was this actually the case? It had been suggested in the lecture that the unequal refrangibility of blue and red light was due to the differences of absorption. This statement seemed very puzzling to many people. It was cleared up by reference to mechanical considerations—to the law of resonance. Let it be supposed that a pendulum is hung from a light and easily-bent wooden frame. Then let a second pendulum be hung from the same frame, and suppose the first-mentioned pendulum to be set swinging. This, said Professor Everett, is the kind of action that takes place between the light waves and the molecules of glass. Those molecules which vibrate in the same time or with the same period as certain light waves, produce a suppression of that colour to which the waves in question correspond. Should the two pendulums imagined be a little out of unison there would still be an effect, the same in kind but different in degree. And just so, if there should be two places in the spectrum marked by a black absorption band—outside the visible spectrum—the absorption would strengthen as these points were approached, and the deviation of light would be increased in a corresponding degree.

Dr. Walmsley said Mr. Rosenhain had very modestly claimed, in the last paragraph of his lecture, that he had only furnished a resumé of leading facts relating to his subject, most of them familiar to those to whom the paper was addressed. He (Dr. Walmsley) could not quite admit the accuracy of that statement. It was a very valuable thing indeed to have the facts brought together as had been done in this instance. On reading the first paragraph of the paper, it might occur to one that to exclude from consideration of the properties of glass "those particularly known as optical," was something like giving the play of "Hamlet" with the part of the Prince of Denmark left out. But it was to be hoped that Mr. Rosenhain would supplement his present paper with one discussing the particularly optical properties of glass upon some future occasion. The phenomenon of devitrification was of considerable importance, and he (the speaker) would be glad to know whether this had been observed at extremely low temperatures, such as those used in experiments with liquid air. The question of hardness must be dealt with by one of the sub-committees on

Optical Standards; but, of course, it was necessary to have a definition of hardness in the first place. One test that had been mentioned in the paper—that of indentation under pressure—was really a test of viscosity. It was a very different matter from the test by scratching with a sharp point. It was not altogether an easy thing to decide precisely what was meant by hardness. Referring to the iodeosin reagent, it would be interesting to learn from Mr. Rosenhain whether its indications were equally applicable in cases where glass was exposed to the corrosive action of various chemical agents different from iodeosin. Did the action of the iodeosin on glass run *pari passu* with the destructive influence of the other agents to which glass instruments or vessels were commonly exposed? An impression that was left by the paper to which they had just listened was, that it was rather a queer thing that glass should answer its optical purposes at all, since it was a material subject to so many different defects.

Mr. Haddon asked whether devitrified glass had been investigated as to its local composition. Had those parts which solidified or crystallised out first a different composition from those solidifying out later? Was not the green tinge observed in many glasses generally due to the presence of iron? And would the lecturer state whether it was a fact that the greenish tinge produced by the presence of iron was less marked in soda glasses than in those containing potash? Nickel steel was a material varying a good deal in composition. The recently introduced nickel steel having a coefficient of expansion of one-millionth, as used for making clock pendulums and such-like purposes, would expand or contract much less than glass under the influence of changes of temperature; and it would not be satisfactory to employ this for making the cells of lenses. With regard to the use of platinum for making joints between metal and certain kinds of glass, it was generally said that platinum was the only metal available for the purpose, since it was the only one with which a leakage of gas could be prevented. Pure platinum, Mr. Haddon believed, answered better than platinum even slightly alloyed. He (the speaker) had sealed copper wires into glass; but for this it was necessary to use a very pure copper, and then, in the opinion of the speaker, there was a certain amount of yielding of the soft metal under those stresses to which the joint

was subjected by the influence of temperature changes. The same thing held good in the case of gold wire, which answered best for sealing into glass when not alloyed.

Mr. Kendal having been called on to speak said that he could add little to what had already been advanced. From a practical point of view it would certainly be easy to attach too much importance to freedom from any trace of colour in glass.

Mr. Dixey said he had been very much interested in the paper read. It treated of matters on which, however, he was glad to learn but unwilling to comment. He had been pleased to hear Dr. Walmsley give expression to one of the difficulties with which they had been met. Hardness was distinct from elasticity, and from viscosity; it was the liability to be scratched. There was another very important question, as to the absorption of different kinds of light. As a spectacle maker and trader he would welcome an improved nomenclature of light rays according to their position in the spectrum—to their wave length. He would ask the scientific experts to lay down rules for the division of light according to its actinic, thermal and other properties. Those furnishing the different varieties of optical glass could then specify them in terms of their absorptiveness for the different kinds of light. The question of tints as applied to spectacle lenses—of the supposed therapeutic effects of colour—was an important one; and standardising was needed here. If we were to arrive at definite ideas on the therapeutic virtues of tinted lenses, we must have clear ideas also as to what different kinds of light are stopped by the several varieties of glass, and as to what effects these coloured lights have separately on the eye. While apologising for the vagueness of the idea, he would suggest that values should be assigned to the absolute properties of glass in terms of the properties of that light which it transmits.

Mr. Thornthwaite said it was quite true, as had been stated, that for many optical purposes little consequence attached to the thorough annealing of glass. That was the case where refraction was concerned; but in glass reflectors for optical purposes it was important that the annealing should be well done. Otherwise in the process of cooling, after leaving the tool on which the glass surface had been worked, some deformation was liable to occur—there was apt to be such an alteration of shape as

would necessitate returning the mirror again and again to the tool. He (Mr. Thornthwaite) had made a great number of "unannealed glasses" by simply bringing the glass to about melting point in the furnace and then removing it and laying on sand to cool fairly quickly, after which beautiful polariscopic effects of colour could be obtaied. The hardness of the external skin of these specimens was such as to prevent the scratching on of a price mark with the diamond. The speaker did not think that pure silver gave any injurious chemical reaction with glass. He was delighted to see that a book written in German, dealing with the subject of optical glass, had been so admirably done into English, and made more understandable to those of us for whom the grandiloquent words of a German text-book were a little troublesome.

Mr. V. H. Mackinney said that he had often observed defects on the lower lenses of the eyepieces of binoculars which had been used in a hot climate—those defects being apt to occur on that lens and that surface of the lens which was furthest from the eye. Sometimes the tree formation was so pronounced that it became necessary to repolish the damaged lens. He would be glad to know the cause of the trouble.

The President then rose to propose a vote of thanks to Messrs. Rosenhain and Chalmers. He said that he had been at first inclined a little to regret that general principles and not practical details had been discussed. However, it was to be hoped that this was only the first of a series of papers by Mr. Rosenhain dealing with the whole subject. It was to be hoped that by the enterprise of Messrs. Chance, the material would be brought together for another book like that which had just been translated by Professor and Miss Everett. This was a thing that we needed and must have in this country; and they must look to Mr. Rosenhain for it. He was the man on whom they had to depend for its accomplishment. Turning to the paper before them, the first matter dealt with was the optical defects of glass. It was the most disheartening thing he (Dr. Drysdale) knew of to find after the tedious work of calculation, and after mechanically working the surfaces of a lens, that the whole result was spoiled by the presence of striæ, which it had been impossible to discover by the ordinary method of examination in the first place. In regard to the explanation of the

absorption bands in glass—it was known to everyone that the violet waves of light vibrated more quickly than the red waves; and flint glass being denser than crown it was natural to suppose a greater impedence being given to the particles in the former case. The denser the glass, the greater would be the check to the more rapid vibrations of light traversing it. If now it was to be inferred from what the lecturer had said that colour is a physical necessity in heavy glass, then it must be useless to experiment any further in the hope, by chemical means, of producing such glass, free from colour. But notwithstanding what Mr. Rosenhain had said, he (the speaker) would welcome a really clear glass. To improve the prism telescope, what was most necessary was to increase the angle of the cone of light; and this requirement demanded the use of glass of higher refractive index—but to retain the quality of transparency was also very important. On the question of hardness, as had been said, that quality had not yet been strictly defined; but from the practical point of view it would not be a very difficult matter to define it. Indentation tests of glass were of little practical utility, as they referred more to the elasticity of the material than anything else. Scratching was a good test, however. Mr. Rosenhain's paper bristled with interesting points. Dr. Drysdale concluded by again expressing the hope that this paper was only the preface to much more work of the same kind, which would elevate the glass industry in this country to the same dignity as it had attained in Germany. The vote of thanks which was then put to the meeting was carried by acclamation.

Mr. Rosenhain, after suitably responding to the vote of thanks, said that he would be glad to give further papers to the Optical Society when he could do so with reference to his own work. He would be sorry to give only abstracts of the work of others. Dr. Hovestadt's book, which had been referred to, was based on the researches of no less than 40 or 50 eminent scientists, who had the resources of the Reichsanstalt University laboratories at their disposal; and it would be asking a lot of him (the speaker) to expect him to give a detailed discussion of more than perhaps one or two points. Probably not more than about half a column of his paper had been occupied with really original matter. Referring to Professor Everett's

remarks, the word "colloid" did not, he was afraid, mean much in relation to glass. He considered that glass might better be described as a fluid, and that it had not the cellular structure characteristic of jellies. Mr. Haddon had mentioned one particular nickel steel alloy—invar—but it was quite possible by varying the proportion of alloy to get all sorts of coefficients of expansion in this material. The difficulty in fusing it into glass would be (as with all except the noble metals) the tendency to surface oxidation. If the size of the annular space that would be left between glass and metal wire of any sort, were calculated for certain variations of temperature, it would be found to be very small indeed. He did not agree with the suggestion that had been made that the use of lead contaminated with silver was responsible for the colouring of dense flints. Silver alone would not colour glass in that way. Again, as the yellow tinge became deeper on heating the glass, it was not likely to be due to the presence of impurity—but rather, as Drude suggested, to altered electrical conditions. As to devitrification of glass at very low temperatures, he had not heard of that. Glass at the ordinary temperature of the air had been known to be practically unaltered after a thousand years. In some cases the mineralogical nature of the crystals separated out from glass could be recognised under the microscope; but there was so much interpenetration that chemical examination was difficult. What the iodeosin test did was simply to measure the solubility of glass in water; and from this a fair deduction could be drawn as to the probable durability of the glass exposed in moist air. The difficulty with Mr. Dixey's suggestion about the classification of light waves consisted in the fact that these overlap so much in respect to their visual, chemical, or thermal properties. Glasses had not been produced, so far, which would only allow of the transmission of one kind of light very definitely. Mr. Mackinney's question could hardly be replied to without information as to the nature of the glass concerned. Perhaps the inside of the binoculars might have been varnished with a varnish containing some dyestuff like iodeosin, which would attack the glass, starting from the comparatively cold edges of the lens near to the metal, as had been indicated by Mr. Mackinney's sketch on the blackboard. By careful microscopic examination, the character of the deterioration might

perhaps be recognised. If Dr. Drysdale wanted something to refract light very strongly and absorb it feebly, he (the speaker) thought that impossible. He remarked further that optical density does not vary with mechanical density, in a regular proportion. He regarded the electrical properties of glass as the most important to be considered in this connection.